INSTITUTE *of* STATISTICS
Oxford

Bulletin Vol. 19 *May 1957* *No. 2*

EDITORIAL NOTE

The August 1956 number of THE BULLETIN contained an article by Dr. M. R. Fisher on savings behaviour. Using cross-section data provided by the Oxford Savings Surveys, Dr. Fisher carried out a detailed investigation of patterns of savings at a household level and considered the influence of factors such as assets, age and family size, as well as income, on the amount saved. In particular, two recent theories of consumer behaviour developed by Professors M. Friedman and F. Modigliani (with the late Dr. R. E. Brumberg) were examined critically in the light of the results obtained from the British surveys.

The authors of these theories, together with several other American and European economists, have been invited to comment on Dr. Fisher's work, and the resulting collection of articles is printed in this issue. While Professors Friedman and Modigliani are mainly concerned with the implications of Dr. Fisher's analyses for their own particular hypotheses, other contributors raise between them a wide variety of theoretical and empirical problems. In order to give some indication of the topics considered, contributors have been asked to furnish titles for their articles, although each article originated as a comment on Dr. Fisher's paper. The discussion is wound up by Dr. Fisher.

TESTS OF THE LIFE CYCLE HYPOTHESIS OF SAVINGS

COMMENTS AND SUGGESTIONS[1]

By **Franco Modigliani** *and* **Albert K. Ando**

Introduction

Dr. Fisher has produced an interesting and ingenious set of tests for some aspects of the hypotheses advanced by Friedman, and Modigliani and Brumberg. So far as we know, this is the first systematic attempt to test these hypotheses, and we are all indebted to Fisher's pioneering effort. Our main concern here will be to assess what light is shed by these tests, and more generally, by the data generated by the Oxford Savings Surveys, on the usefulness of the Modigliani-Brumberg model of consumer behaviour.

Our analysis is divided into three parts:

(1) We first attempt to evaluate the reliability and adequacy of the data employed by Fisher, both from conceptual and statistical view points. Clearly, such an evaluation is essential to get some perspective as to how much weight should be given to any results, favourable or unfavourable to our model.

(2) We then proceed to examine the behaviour of the observed saving income ratio as a function of age, in relation to the behaviour that is to be expected from the Modigliani-Brumberg (M-B) life cycle model. For this purpose, we can rely not only on the 1953 data utilized by Fisher, but on the evidence provided by the 1952 and 1954 survey data as well.

(3) Finally, we analyze the results of Fisher's cross-section correlation tests. As a first step in this analysis we have proceeded to spell out in some detail the quantitative implications of the M-B model, with proper allowance for the role of family size and the effect of systematic variations in income over the earning span. We then compare the observed behaviour of the marginal propensities to save with respect to income, assets, and family size at various ages with the behaviour implied by our model. In so doing we point out the many critical shortcomings of Fisher's tests and suggest alternative sets of tests. We argue that these alternative tests are far more relevant in assessing the acceptability and usefulness of the M-B model, and are largely feasible on the basis of the data provided by the Oxford Savings Survey.

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[1] The analysis contained in this comment has been carried out as part of our contribution to the project, 'Consumer Expenditure Study' of the Wharton School of Finance and Commerce, The University of Pennsylvania, supported by a grant from the Ford Foundation.

I. The Adequacy of the Data

We must first observe that, although a sample of more than 2,000 observations appears to be quite large at first glance, by the time we cross-classify the observations by age, occupation and income, frequencies in individual cells soon become uncomfortably small. Since we are not dealing with controlled experiments, it is very doubtful whether a sample of as few observations as, say, thirty, can give us any meaningful result. Furthermore, when we are dealing with expected values of variables and their ratios, there is no reason to eliminate the extreme observations as Fisher has done, although he may be quite justified in doing so for the purpose of regression analysis. These points are particularly serious because of the extreme variability within each cell of such key variables as saving, and the consequent high sensitivity of expected values to the omission or inclusion of a few observations. This point is well illustrated in Table 1.1. below which compares, for each age group, average saving for the observations utilized by Fisher with average saving for all observations in the survey. The discrepancies would of course be magnified if we further subclassified the data by other criteria, such as occupation. The instability of these averages is further emphasized by comparing the estimate of average savings by age for three consecutive years.

Table 1.1

Comparison of Average Savings: Total 1953 Sample and the Sample Italized by Fisher

(1)	(2)	(3)	(4)	(5)
Age	*Savings 1953 (Fisher)*	*Savings 1953 (Total Sample)*	*Number of Observations*	*Number of Extreme Observations Eliminated by Fisher*
18—24	2	3	222	2
25—34 ...	0	4	362	2
35—44	14	15	407	5
45—54	3	6	371	9
55—64	7	8	260	5
65 and over ...	—17	—13	381	17

On the basis of these considerations we feel that we should not pay too much attention to the results obtained in any particular cell but try to look at the picture as a whole. We suggest for instance that much of the effort devoted by Fisher ingeniously to rationalize the peculiar behaviour of certain coefficients—witness his analysis of the self-employed group—may well be wasted. For, we rather suspect that such peculiarities will not reappear, or will be replaced by quite different ones, if the analysis is repeated for different samples of the same size.

Let us now turn to a consideration of discrepancies between the definitions of saving and net worth implicit in the M-B model and those used in the survey. Strictly speaking, the concept of saving relevant to the M-B model is the change of net worth of the household during the period. The Survey basically measures saving by estimating the increases and decreases of the various assets and liabilities of households, so that if this procedure were adhered to consistently, we would have had an ideal measure of saving. Unfortunately, however, this is not at all the case because of deficiencies in the definition of net worth used in the Survey, and because the definition of saving does not coincide with the first differences in net worth. From the point of view of saving, the most serious shortcoming is in the treatment of durables. This inadequate treatment introduces a systematic bias in the analysis of the effects of age on saving as acknowledged by Fisher and discussed further below.[1] As for net worth, while we are duly impressed by the courageous attempt at measuring this variable, we are puzzled by the failure to include in it the cash surrender values of life insurance policies and individuals' shares in retirement and pensions funds, both private and public. It is not at all clear why these two very important items are omitted, particularly in view of the fact that insurance premiums and employee contributions to retirement and pension funds are included in the Survey definition of saving. It is stated in various reports of the Survey that the payments by individuals into these funds are taken as a part of saving in place of changes in their shares in these funds, because it is difficult to ascertain such changes accurately. However, even granting this difficulty, it should be possible to secure a rough estimate of the level of their equity in these funds.[2]

By far the most puzzling feature of the data obtained by the Survey is the apparent difficulty of reconciling the estimates of net worth with the surprisingly small estimates of saving. While this difficulty is by no means limited to the Oxford Surveys, it reveals itself most strikingly in this survey, thanks to the comprehensive coverage of the components of saving and net worth and the careful definitions employed.

TABLE 1.2
Net Worth, Actual and Computed by Accumulating Saving

(1)	(2)	(3)	(4)	(5)
		Net Worth Implied by Saving		
Age	*1953 Net Worth*	*1953*	*1952*	*1954*
18—24	40	11	0	28
25—34	320	41	25	146
35—44	760	136	25	306
45—54	1,160	246	185	471
55—64	1,200	341	205	636
65 and over ...	(1,180)			

The difficulty referred to above can best be appreciated by performing the experiment shown in Table 1.2. In column (2) we give net worth by age as estimated in the 1953 Survey. In column (3) we exhibit net worth by age as 'computed' or inferred from the information on saving by age reported by the same survey. The figures in the column (3) are obtained by assuming (i) that the net worth of a household at the beginning of the earning span is zero; (ii) that the average amount saved by households at any given age is the same as the average amount saved in 1953 by the households of the corresponding age[1]; (iii) that the average net worth of any age group is the net worth at the beginning of the age group plus half of the accumulation for that age group. For instance, since in 1953 average savings for age group 1 is £3 per year, a household would accumulate net worth of £21 by the time it is 24 years old. Thus, the average net worth for the age group 1 in this case is £11. Between 25 and 35 the household saves on the average £4, accumulating £40 during this period. Hence, the average net worth for the second age group is, according to our method of calculation £41, and so on. Admittedly, this is a very rough calculation, and a slight change in our assumptions may result in substantial changes in the estimates of net worth at various ages. However, the total accumulation of net worth over the total earning period would not be substantially affected. In columns (4) and (5) we exhibit the results of similar computations based on the estimates of saving by age given in the 1952 and 1954 survey respectively.

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[1] The inclusion in saving of the gross purchases of durables in some of Fisher's regression analysis generates a further bias because of his use of liquid assets as a proxy for net worth. See on this point Section III, pages .

[2] We have no way of knowing how important these items are in the balance sheet of households. For the United States in recent years, these items constituted about ten per cent of total assets of the households on the aggregate, or about 12 to 13 per cent of the net worth of the households. (See, for instance, R. W. Goldsmith [3], Volume 3, Table W-46.)

If we compare these results with the actual distribution of net worth for 1953, we see that the discrepancies are quite striking. Even for the year 1954 when the reported savings were highest, the accumulation of saving over the life span accounts for but one half of net worth held by the households in the age group 5. In the case of 1952, it is only one sixth.

There are basically three factors that can account for these discrepancies, namely: (1) conceptual differences between the survey definitions of savings and net worth; (2) changes over time in the savings habits of households at various ages; (3) statistical discrepancies, *i.e.*, inaccurate reporting by respondents and/or sampling errors.

The differences between the survey definitions of savings and net worth have been treated by several authors, and are summarized in the following formula taken from Hill with slight modifications.[2]

Change in net worth = Saving

plus

Capital gains on securities and property
Gifts and other capital transfers received, including bequests
Lump sum benefits from insurance and retirement and pension funds
Value of cars owned at end of year
Receipts from any excess of sales over purchases of durables.

minus

Capital losses on securities and property
Donations and other capital transfers
Value of cars owned at beginning of year
Payments on life insurance
Superannuation contributions
Amount paid for any annuities purchased

For 1953, we can make rough adjustments for most of these definitional discrepancies, using the information given in various reports on the Survey.

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[1] Throughout this paper, the age grouping is the same as that given by Fisher, and each age group will be denoted by the numbers given to them by Fisher.

[2] Hill, T. P., [4].

We have direct information on the values of cars owned,[1] payments on life insurance,[2] superannuation contributions,[2] etc., while large gifts, lump sum insurance benefits, and bequests received, are included in the Lump Sum Receipts given by Klein.[2] The various adjustments, which are based on the same procedure utilized in Table 1.2, are shown in Table 1.3. For the lump sump receipts we cannot exhibit the life pattern because of lack of data, but from the data given by Klein we can make a rough inference as to the amount received on the average by a household over its earning span.[3] After all the major definitional adjustments are made, accumulated savings account for only about one half of the net worth holdings reported by the households in age group 5.

Table 1.3

Reconciliation of Net Worth With Saving, 1953

(1)	(2)	(3)	(4)	(5)	(6)
Age	*Net Worth Less Automobile*	*Net Worth Implied By Saving*	*Insurance Premiums and Super Annuation*	*Lump Sum Receipt*	*Implied Net Worth*
1	36	11	—1	935	
2	309	41	—11		
3	725	136	— 7		
4	1,124	246	—15		
5	1,172	341	— 3		593

To some extent this huge discrepancy may result from the fact that our conceptual reconciliation is not complete. In the first place, Klein's figures underlying column (6) presumably do not include gifts and bequests received in forms other than cash. We do not know the size of the bias caused by this omission, but it does not seem reasonable that it could be large enough to distort the picture to the extent shown in Table 1.3. In the second place, we have made no adjustment for the substantial capital gains which have occurred at least since World War II. However, if we are willing to assume that prices of non-money fixed assets have moved roughly in step with the general price level, no major adjustment needs to be made in our reconciliation on this account. This is because past savings were made at price levels lower than that prevailing in 1953. Insofar as these past savings were embodied in non-money fixed assets, and the price level of these assets moved in proportion with the general price level, the capital gains simply serve to revalue the past savings to the price level of 1953, which underlies the computations of column (3) to (7). If we allow for the fact that a part of past savings must have taken the form of money fixed assets, the net worth figure should actually be lower than that implied by current saving. On the whole, therefore, these two remaining sources of conceptual discrepancies do not seem sufficient to account for the large difference observed in Table 1.3.

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[1] Straw, K. H. [9]. [2] Klein, L. R. [5].

[3] The amount shown in column (5) represents £19 .9, the estimate of the average lump sum cash receipt per household in 1953, multiplied by the number of years in the earning span which is generously taken as 65 − 18 = 47. The figure of £19.9 is in turn computed by multiplying £186, the amount of lump sum receipt averaged over the households receiving it, by 0.107, the proportion of households receiving such payments. (Klein, [5], page 206).

Another possible explanation for the discrepancy might be found in the changes in savings habits over the past few decades. If such changes have occurred, then the current cross section profile will not agree with the life history of the average household now in a given age group. But in view of the size of the discrepancy observed in Table 1.3, the changes in savings habits must be radical indeed, and should manifest itself in a marked reduction over time in the ratio of aggregate saving to aggregate income, net of taxes. Now, by and large, the analysis of historical data for many countries has indicated a remarkable long run stability of the saving income ratio. This stability, at least in the case of the United States, has been dramatically confirmed by the recent monumental work of R. W. Goldsmith [3].

To be sure, this long run stability was interrupted by extremely high savings during World War II. However, the high savings of the war years are to a large extent off-set by the unusually low savings during the 1930s. On the whole, at least in the case of the United States for which we have easier access to the data, the high savings during the war cannot possibly account for as large a discrepancy between saving and net worth as found in Table 1.3.[1]

It is quite possible that in Great Britain savings habits in recent years have changed much more drastically than in the U.S., as a result of the changes in the social structure. However, from a cursory examination, aggregate British income and savings data do not seem to support the hypothesis that a radical change in saving habits provide an adequate explanation for the discrepancy we have uncovered, though we do not feel that we are in a position to pass any final judgment.[2] We, therefore, wish to call attention to what seems to us an important and difficult problem of reconciliation, not so much as a criticism of the Oxford Surveys, but rather in the hope that other analysts with more intimate knowledge of the data might be able to throw some light on this intriguing problem.

For the moment, however, we are inclined tentatively to conclude that the discrepancy observed in Table 1.3 results to a substantial extent from inaccurate reporting from the respondents. Furthermore, we feel that the net worth estimates should tend to be more accurate than the estimates of savings, which represent the first differences of various assets and liabilities. This would imply that the survey data tend to underestimate substantially the level of saving.

[1] A similar comparison made using the aggregative net worth data given by Goldsmith (3) and savings data provided by the survey conducted by the Bureau of Labour Statistics for 1950, though very tentative and rough, seems to indicate a discrepancy even greater than that shown in Table 1.3.

[2] According to the data published in the *Annual Abstract of Statistics* of the Central Statistical Office, the saving/income ratio for 1938 was about 5.5 per cent, whereas the average of saving/income ratios for 1951 to 1955 was about 5.6 per cent. This comparison is rather crude, but *prima facie* suggests that there has been no drastic change in savings habits.

In the light of these considerations, in the analysis that follows we propose to concentrate on the pattern rather than the level of saving on the assumption that the downward bias of the savings estimates, if any, affects primarily the levels of the estimates while the pattern is not significantly affected.

I. THE LIFE CYCLE OF THE SAVING INCOME RATIO

The cornerstone of the M-B model is the notion that the purpose of saving is to enable the household to redistribute the resources it gets (and expects to get) over its life cycle in order to secure the most desirable pattern of consumption over life. As was shown in [8], in the special case where (i) the preferred pattern of consumption calls for a constant rate of consumption over life; (ii) income, actual and expected, is constant over the earning span, and (iii) variations over life in family size are neglected, the M-B model implies that the ratio of saving to income should be a positive constant at every age during the earning span, and a large negative constant during each year of the retirement span. If, however, one or more of the above three assumptions are not satisfied, the life pattern of the saving income ratio will depend on the life profiles of family size and income, and on the preferred pattern of consumption over life.

The data collected by the Oxford Savings Survey show that in the United Kingdom, as in the case of the U.S. and other western countries, average income for the population as a whole traces a roughly parabolic path over the earning span. It rises very sharply from age 1 to 2,[1] reaches its shallow peak at age 3 or possibly 4, and then declines but only moderately at age 5.

The profile of the family size is also parabolic, but has a considerably more pronounced peak unmistakeably located at age 3, and furthermore the decline from this peak is much more rapid than decline of income. Accordingly, the cycle of average income per member of the household traces a cycle almost inverse to that of income, reaching its bottom at age 3 and then rising markedly at age 4 and again at age 5.

The implications we can draw from these data within the framework of our life cycle model are fairly clear. Unless the preferred consumption per head of the household is significantly different at different ages within the earning span, we should expect that much of the saving necessary for financing the dissaving after retirement should tend to occur around age 4 and 5 when the pressure of family size has considerably subsided while income is still relatively high.

In order to illustrate this point let us suppose for a moment that the preferred pattern of consumption over life involved a constant expenditure per member of household per year. Under this assumption the life pattern of consumption would be identical with that of family size. This pattern is shown in column (3) of Table 2.1. It is obtained by averaging the estimates of family size by age reported respectively by the 1952 and 1953 surveys, and it is expressed in the form of an index in which the average value for the age groups 2 to 5 inclusive is taken as 100. This hypothetical consumption profile can be compared with the life cycle of income—the path of average income of successive age groups—estimated on the basis of data from the Saving Survey for 1952 and 1953,[1] and shown in col. (2).

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[1] Throughout this paper, age groups will be denoted by the numbers given to them by Fisher.

A comparison of the two columns reveals that at age 2 income is below average while family size is above average. At age 3, both income and family size are above average, but family size much more than income. At age 4, on the other hand, size falls to the average while income is still substantially above. Finally, at age 5, the drop in family size greatly outstrips the fall in income. On this basis we should expect saving at age 3 to be at a low ebb as compared with age 4 and 5. The same should tend to be true at age 2, especially if saving is defined to exclude additions to the stock of durable goods. Such additions are in fact likely to be most important during the formative stages of the household which will typically occur in this age bracket. The higher level of savings of age groups 4 and 5 as compared to 2 and 3 should be even more pronounced in terms of the saving-income ratio, since income is on the whole lower in the last two age groups.

The behaviour of income and saving in age group 6 cannot be adequately gauged from the survey data since many of the receipts which are classed as income by the survey should really be regarded not as income but as liquidation of assets or claims, as Fisher has repeatedly pointed out in his paper. However, even if we accept the Survey definition we should expect saving to be low (or dissaving to be large) relative to most other age groups.

It should be recognized however that the figures of column (3) may well tent to exaggerate the influence of family size on the desired consumption pattern. That is, as family size increases, the consumption expenditure necessary to maintain a given standard of living will tend to rise proportionately less. For instance, the cost of a given standard for a family of four may be not four times, but, say, only two and a half times as large as for a household of one. A household of four would then represent 2½ ' equivalent ' members. The maintenance of a given standard of living over life would then imply a constant expenditure not per member but per ' equivalent member '.

The problem of establishing ' equivalent units ' has long been under investigation and we cannot touch upon it in this brief comment.[2] But for the sake of illustration let us follow Fisher's suggestion that the ' cost ' of a household of size J is proportional to the quantity $1+a(J-1)$, where a is some constant—i.e., that all members beyond the first have a constant ' marginal cost '. In deriving the figures of column (3) we have in effect assumed that a was unity which is presumably too high. In column (4) we have recomputed the life pattern of consumption assuming for a a value of ½. This value is almost certainly too low (except possibly for very large

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[1] For details on the method of estimation see footnote to Table 2.1.

[2] See, for instance, Friedman, M.[2].

TABLE 2.1

The Life Patterns of Saving Income Ratio[1]

(1) Age Group	(2)[2] y (1952–53)	(3)[3] C	(4)[4] C'	(5)[7,5] $\frac{S}{y}$ (Three Surveys)	(6)[7,6] $\frac{S}{y}$ (Fisher)	(7)[8,5] $\frac{S'}{y}$ (Three Surveys)	(8)[8,6] $\frac{S'}{y}$ Fisher	(9)[9] $\frac{S^c}{y}$ $S^c = 37.3 + .102y$	(10)[9] $\frac{S^c}{y}$ $S^c = -33 + 112y - 3.41J$	(11)[9] $\frac{S^c}{y}$ $S^c = -29.5 + 0.137y - 5.91J - 0.040I.$
1	54	54	60	60	62	71	63	—245	—290	— 85
2	97	104	102	81	2	118	134	100	85	142
3	111	127	120	77	202	100	121	145	135	158
4	109	101	101	138	43	108	85	140	140	121
5	82	68	77	104	152	74	60	15	45	— 21
6	47	54	67	—242	—597	— 31	— 64	—350	—320	—474
4[10]	106	102	101	(168—320)	100	(124—125)	98	130	135	116
5[10]	85	75	82	(200—219)	320	(107—116)	93	35	60	63

[1] All figures shown are indices where the average value for age groups 2—5 inclusive is taken as 100.

[2] Based on simple averages of the net incomes for 1952 [6] and 1953 [1], where the former is adjusted for the growth of per capita income to be made compatible with the latter. This income is used for calculation throughout this table.

[3] Assuming constant expenditures per member and based on simple averages of family sizes for 1952 [6] and 1953 [1]. See Text, pp. 105, 106.

[4] Assuming constant expenditures per equivalent adults = 1+(J—1)×.5, and same family size as in column (3). See Text, pp. 106, 108.

[5] Average of savings for 1952, 1953, 1954 reported respectively in [6], [1], and [5]. See Text, p. 108.

[6] Savings reported by Fisher [1]. See Text, pp. 108, 109.

[7] Excludes expenditures on durables.

[8] Includes expenditures on durables.

[9] Equations are taken from Fisher [1], Appendix A, Table VI. See Text, p. 110.

[10] Excluding retired and unoccupied. The base of the index for these rows are same as that for rows 1-6. See Text p. 109.

households) but it will give us a sort of a minimal estimate of the effect of the variation of family size over life. Actually a comparison of columns t3) and (4) shows that even halving *a* does not affect drastically the life pattern. In the four crucial central age groups, 2 to 5, the effect is quite moderate and merely attentuates the sharp peak at age 3. In the groups 1 and 6 there is an appreciable relative increment, which seems quite reasonable.

It is apparent that on the whole the inferences drawn on the basis of column (3) are still tenable. On the basis of a life cycle model, and as long as we are willing to make the assumption that, on the average, the preferred pattern of consumption calls for maintaining roughly the same standard of living over life, we should expect the following: as between the four earning span groups, 2 to 5, the saving ratio should tend to be lowest at age 3, highest at age 4 for 5; for age 2 it should be larger than for age 3 as long as the addition to the stock of durables is included in saving. If it is excluded it might still be larger but the conclusion is less certain. Finally at age 6 substantial dissaving (or at least very much smaller saving) should occur.

We shall now examine how well these expectations are met by the Survey data. We exhibit in column (5) the life pattern of the saving-income ratio (excluding durables) computed by pulling together the information provided by the three Saving surveys of 1952, '53 and '54. Saving for each group was obtained by averaging the figures for the three surveys, while the income series is the one underlying the index of column (1). We feel that, in view of the marked variability of the estimates between the three consecutive surveys, pooling of the data is definitely the safest course of action, in spite of the crudeness of our method.

The figures of column (5) conform reasonably well to our expectations. The saving ratio is lowest at age 3 and nearly as low at age 2. At age 4 and 5 it is on the average nearly 60 per cent higher than in age 3. At age 6 a large dissaving occurs. Actually, as will be indicated below, the figures for age group 5—and to some extent even for age group 4—underestimate considerably the savings ratio which is relevant to the testing of our model. This is because, for age groups 1–5, our model relates to non-retired households while in the Survey data the samples relating to age groups 4, and especially age group 5, contain a considerable number of retired households.

Before we attempt to make any corrections for early retirement, it is useful to compare column (5) with column (6) in which we present the saving pattern implicit in the data used by Fisher in his extensive correlation tests. The lack of similarity between the two columns could hardly be greater! For the critical age groups 2 to 5 the peak in column (6) coincides precisely with the trough in column (5) while the peak of (5) corresponds to the second smallest figure in column (6). To some extent these amazing discrepancies reflect the fact that the life pattern of the saving ratio in the 1953 survey is actually at odds with the remaining two. In both the 1952 and 1954 surveys, the peak of saving is clearly in class 4, and saving in group 3 is the smallest. The monitoring of the data carried out by Fisher contributes to the discrepancies even further. Whatever the sources of the discrepancies, and without entering into the merit of monitoring—which we do not wish to criticize in principle—it is apparent that conclusions based on Fisher's sample as to the systematic effect of age on saving behaviour must be taken with considerable reservations, to say the least.

In spite of these limitations, Fisher's data may be helpful in providing some idea of the effect of the inclusion of the retired households on average savings at age 4 and 5.

To this end we have recomputed, with the help of the data given by Fisher in Appendix A and C, income and saving in each age group excluding households reported Retired and Unoccupied.

As is expected, the elimination of the retired and unoccupied group has a negligible effect on age groups 1 to 3. In class 4, however, income increases about 4 per cent, but saving more than doubles; and in group 5 income increases nearly 20 per cent and saving about 2½ times. As a result in both groups the saving income ratio more than doubles. This is shown in the last two rows labelled 4′ and 5′ in column (6) where the saving income ratio of the non-retired households is again expressed as percentage of the average ratio of all households in groups 2 to 5.

It is impossible for us to compute directly corresponding figures for row 4′ and 5′ in column (5), since we do not have at our disposal the required data for the '52 and '54 surveys. We have however computed two alternative rough estimates which are given in parenthesis in column (5). While these estimates are far from reliable they do suggest that with the elimination of retired households the saving income ratio in class 4 and 5 might tend to be roughly twice as large as in groups 2 and 3 and that the peak ratio might even be shifted from group 4 to 5.

The saving concept underlying columns (5) and (6) is the traditional one which does not include net additions to the stock of durables. Since no information is available as to the behaviour of saving as defined in the M-B as well as Friedman model, it is worthwhile to take a look at the age pattern of gross-saving, *i.e.*, traditional saving plus gross purchase of durable goods, for which information is available. In column (7) we exhibit the estimated behaviour of the gross-saving/income ratio, computed again by pulling together the information provided by the three Saving surveys. For comparison, in column (8) we present the series obtained from Fisher's 1953 sample.

Once more the discrepancy between the figures in the two columns is remarkable although it is a good deal less pronounced than for the alternative definition of saving. Looking at the figures of column (7) we observe again that the saving ratio dips at age 3. However, the recovery at age 4 is very weak and a further pronounced fall appears at age 5, so that the peak of the ratio occurs at age 2. The behaviour of the last two groups, however, appears again to be considerably affected by the inclusion of the retired households. Estimates of the behaviour of non-retired households, computed along the lines described in connection with column (5), are shown in rows 4′ and 5′. After this rough correction, the fluctuation of the gross saving income ratio is much less than that of the net saving income ratio, and probably somewhat smaller than might be suggested by the life cycle model. Also, the figure at age 5 remains disappointingly low. Nevertheless, the trough of the ratio still appears to occur at age 3 and its peak in the latter part of the earning life cycle. Pending the availability of more appropriate estimates of saving for the non-retired households, we may, therefore, conclude that the age pattern of the saving ratio is not inconsistent with what we might expect on the basis of the life cycle model.

It is important to point out that this observed behaviour of the saving ratio cannot be readily accounted for by the more traditional models of saving behaviour which ignore the life cycle, or, more generally, the effect of age.

Consider first the most popular and widely accepted model according to which saving behaviour can be adequately explained by current income, and the saving ratio is an increasing function of current income. According to this model—referred to hereafter as the income model—the saving income ratio should reach its peak at age 3 where income has its peak, while for group 5 it should be distinctly lower than for any other group in the range 2 to 5. In fact as we have seen, the ratio has its trough in group 3 and has its peak, or virtual peak in group 5. The utter inadequacy of the income model and the significance of the age variable is probably best brought out by a comparison of the figures of column (5) with those of column (9). The figures in this column were computed from the regression equation of saving on income given by Fisher for his entire sample and reported in Appendix A, Table VI.

$$S = -37.3 + .102y.^{*} \tag{2.1}$$

The discrepancy between the pattern of column (9) and the actual age pattern of column (5) is so striking as to require no further comment. The results are not very different if we abandon the simple income model and take into account family size or even liquid assets. This conclusion is supported by the figures in column (10) computed from Fisher's regression of saving on income and family size, and in column (11) computed from his regression of saving on income, family size and liquid assets. It is apparent that neither of these models is capable of accounting for the observed pattern of column (5), especially for the critical groups of 3, 5, and 5′.

In summary, the behaviour of the saving income ratio as a function of age as revealed by the Oxford Surveys seems to bear a fair relation to the pattern implied by the life cycle model. The same is largely true of the behaviour of net worth over life. This conclusion has in fact been already

[*] For each group we first obtained a 'computed' saving by substituting for y in (2.1) the average income of the age group, estimated from the '52 and '53 surveys, as in Column (1), except in row 4′ and 5′ for which we had to use the actual level of income exclusive of retired and unoccupied households in 1953, as computed from Fisher's data. S was then divided by y and the resulting ratio expressed as a per cent of the average ratio of groups 2 to 5. Ideally the regression equation for this test should have been based on all three surveys appropriately pooled. But while such a refinement might have affected the level of S^c, it clearly could not significantly affect the overall pattern in column (9).

stated very explicitly by another analyst of the same data.[1] On the other hand, the observed patterns of saving and net worth are not readily explainable by more traditional alternative hypotheses which endeavour to explain saving behaviour entirely on the basis of current variables without reference to the life cycle.

III. Marginal Propensities

In analyzing the behaviour of the marginal propensities, we must rely entirely on Fisher's data. The limitation of this set of data has been adequately demonstrated in the preceding sections, and we shall therefore proceed with due caution.

As Fisher has correctly pointed out, it is necessary to introduce family size explicitly into the M-B model before we can derive inferences from it which are useful in connection with the Fisher tests. In so doing, we follow the formulation advanced by Fisher (see pp. 224–226 of his article). While more refined formulations can readily be elaborated and will in fact be discussed in a forthcoming study,[2] we feel that for the purpose of the present comment Fisher's approach represents an acceptable approximation.

We accordingly take as our starting point Fisher's equation (2.13), or,

$$C^t_\tau = \gamma^t_\tau V_t = [\gamma''^t_\tau + \beta''^t_\tau\,(J^t_\tau - 1)]\,V_t. \tag{3.1}$$

Here V_t denotes the total resources at age t, *i.e.*,

$$V_t = Y_t + A_t \tag{3.2}$$

where A_t is the net worth at age t and Y_t is the total income from gainful employment anticipated until retirement.

In his analysis, however, Fisher has failed to note the implications of his approach concerning the behaviour of β^t_τ as a function of age. To see clearly the nature of this dependence let us first write

$$\beta''^t_\tau = \alpha^t_\tau\,\gamma''^t_\tau \tag{3.3}$$

which merely defines a new coefficient α^t_τ. Fisher's equation (2.12) then becomes

$$\gamma''^t_\tau = \gamma'; \quad \beta''^t_\tau = \beta' = \alpha\gamma' \tag{3.4}$$

which implies

$$\gamma^t_\tau = \gamma'\,[1 + \alpha\,(J^t_\tau - 1)] \tag{3.5}$$

It is apparent that the coefficient α has the very same meaning as in Section II.

Using (3.5), Fisher's (2.13) becomes

$$\gamma' L_t\,[1 + \alpha\,(\bar{J}^t - 1)] = 1$$

where $L_t = L + 1 - t$ and L is the length of life. This implies, together with (3.4),

$$\gamma''^t_\tau = \frac{1}{L_t\,[1 + \alpha\,(\bar{J}^t - 1)]}\;; \quad \beta''^t_\tau = \frac{\alpha}{L_t\,[1 + \alpha\,(\bar{J}^t - 1)]} \tag{3.6}$$

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[1] Lydall, H.F., [7], especially pp. 149–150.

[2] This study is being prepared in connection with the 'Consumer Expenditure Study' at the University of Pennsylvania referred to earlier.

where $\bar{J}^t$ is average family size over the remaining life of the household. The substitution of (3.2) and (3.6) into (3.1) then yields

$$C^t_\tau = \frac{1 + \alpha(J^t_\tau - 1)}{L_t[1 + \alpha(\bar{J}^t - 1)]} (Y_t + A_t) \tag{3.7}$$

Using c_t and J_t as short hand notations for C^t_τ and J^t_τ, and denoting by p_t the permanent component of income (in the Friedman sense), the current consumption function can be written as

$$c_t = \frac{Y_t/p_t}{L_t}\left\{\frac{1+\alpha(J_t-1)}{1+\alpha(\bar{J}^t-1)}\right\} p_t + \frac{1}{L_t}\left\{\frac{1+\alpha(J_t-1)}{1+\alpha(\bar{J}^t-1)}\right\} A_t \tag{3.8}$$

The variable p_t is not directly observable. It can however be expressed in terms of the observable current income, y_t, by following the formulation given by Modigliani and Brumberg. That is, we assume that the current income of a household during period t is distributed around the permanent income of the previous period, *i.e.*,

$$y_t = \lambda^{(t)} p_{t-1} + \epsilon_t \tag{3.9}$$

where ϵ_t is a disturbance with expected value of zero, and $\lambda^{(t)}$ is introduced to allow for systematic variations from year to year due, *e.g.*, to the change in age.[1] This implies that we have the inverse relationship

$$p_{t-1} = \mu^{(t)}_0 + \mu^{(t)}_1 y_t + \eta_t \tag{3.10}$$

where $\mu^{(t)}_1 = \frac{1}{\lambda^{(t)}} r^2_{y_t p_{t-1}}$; $\mu_0 = \frac{1}{\lambda^{(t)}} \bar{y}_t \left(1 - r^2_{y_t p_{t-1}}\right)$

and η is the disturbance with $E(\eta_t) = 0$*

Next, we assume that the currently expected permanent component of income, p_t, is a weighted average of current income and the permanent income of the previous period, *i.e.*,

$$p_t = w^{(t)}_1 y_t + w^{(t)}_2 \lambda^{(t)} p_{t-1} \; ; \quad w_1 + w_2 = 1\dagger \tag{3.11}$$

Substituting for p_{t-1} from (3.10) we can write

$$p_t = v^{(t)}_0 + v^{(t)}_2 y_t \tag{3.12}$$

where the coefficients v_0 and v_1 are given by

$$v^{(t)}_0 = \left(1 - r^2_{y_t p_{t-1}}\right)\left(1 - w^{(t)}_1\right) \bar{y}_t = v_0{}'^{(t)} \bar{y}_t \tag{3.13a}$$

$$v^{(t)}_1 = r^2_{y_t p_{t-1}} + w^{(t)}_1 \left(1 - r^2_{y_t p_{t-1}}\right) \doteq r^2_{y_t p_{t-1}} \left(1 - w^{(t)}_1\right) + w^{(t)}_1 \tag{3.13b}$$

---

[1] This formulation is not identical with that given in [8], in which the expected income of the previous period y^e_{t-1} was used in place of p_{t-1}. However, it is an obvious extension which is more suitable once we drop the assumption that income is expected to be constant over the balance of earning span.

* As noted in [8], it might be preferable to state the assumption (3.9) in the form

$$y_t = \lambda^t p_{t-1}(1 + \epsilon'_t)$$

in which case the regression of p_t on y_t is not linear and homoscedastic. However, we do not feel that, for our present purposes, it is essential to pursue the implications of this refinement. It will be developed in some detail in our forthcoming paper.

† w_1 can be interpreted as a measure of elasticity of expectation, and is presumably an increasing function or r^2. See [8], pp. 413–414.

and $\bar{y}_t$ is the mean value of y taken over all households of age t. Note that (3.13a) and (3.13b) imply $\bar{p}_t = \bar{y}_t$.

The substitution of (3.12) into (3.8) finally gives us

$$c_t = \frac{1}{L_t} H_t G_t (v_0^{(t)} + v_1^{(t)} y) + \frac{1}{L_t} G_t A \tag{3.14}$$

$$= \frac{1}{L_t} H_t G_t v_0^{(t)} + \frac{1}{L_t} H_t G_t v_1^{(t)} y + \frac{1}{L_t} G_t A$$

where

$$H_t \equiv \frac{Y_t}{p_t} \tag{3.15a}$$

$$G_t \equiv \frac{1 + \alpha (J_t - 1)}{1 + \alpha (\bar{J}^t - 1)} \tag{3.15b}$$

Here H_t represents the ratio of aggregate income over the balance of life to the permanent component of current income, and G_t is the ratio of the number of equivalent members currently in the household to the expected average number of equivalent members.

Thus, we see that, according to our model, current consumption should be a function of age, current income, net worth and family size, and of prospective income and family size.

We are now ready to examine how we can use the data obtained through the Oxford Savings Survey in order to test our model with respect to the behaviour of the marginal propensities to save.

We must first note that of all the relevant variables the only ones which the Survey has attempted to measure are current income, current family size, age and current net worth and its components. From the equations (3.14) and (3.15) we see that the consumption of the household can be regarded as a linear function of current income and net worth. However, the coefficients of these variables as well as the constant term for a given household are fairly complicated functions not only of observable age and family size, but also of the unobservable variables J and p. Because of the complex way in which the age variable enters into the equation and interacts with all other variables, meaningful tests of the effects of age can best be carried out by stratifying the sample by age. The estimated coefficient for various age groups can then be compared with the theoretical expectations implied by our model. Estimates of these coefficients for six age groups in various occupational categories are actually provided by Fisher (see especially the tables in Appendix) using various alternative sets of independent variables.

It is convenient to analyze first the estimates obtained by Fisher using as independent variables current income and liquid assets. Conceptually, our model calls for the use of net worth and not liquid assets. Hence, in analyzing the implications of our model, we shall assume for the moment that net worth rather than liquid assets is used as an independent variable. The effects of substituting liquid assets for net worth will be discussed later in this section.

The regression of c on y and A for a given age group yields an equation of the following form:

$$c_t = Q_0^{(t)} + Q_y^{(t)} y + Q_A^{(t)} A + u_t \tag{3.16}$$

By comparing this equation with (3.14) we can infer that $Q_0^{(t)}$ should be roughly equal to the mean value of the quantity $H_t G_t v_0^{(t)} / L_t$ for all households in the sample. Similarly, $Q_0^{(t)}$ and $Q_A^{(t)}$ should be approximations to the mean values of $\frac{H_t G_t v_1^{(t)}}{L_t}$ and G_t/L_t respectively.[1]

Our model provides some indication as to how these coefficients should behave over the different ages. Consider first the coefficient of assets, which has the simplest form. For a household in any given age group, we can compute the factor $1/L_t$, by making some reasonable assumption about the average length of life, say, 75 years. For the remaining factor, G, the numerator, as we can see from the definition (3.15b), is directly observable since we know the average family size for a given age group. On the other hand, the denominator involves $\bar{J}^t$ which is not directly observable. We can, however, get an approximation by making the assumption that, on the average, the family size expected at age τ by households of age t smaller than τ is equal to the average family size of the households currently of age τ. We can then compute $\bar{J}^t$ by averaging the average family sizes of the age groups older than t. In the Survey data, we can apply this procedure only roughly because the age classification is coarse and the last age group is open ended. In view of our assumption on the average length of life just stated, and because each age group 2 to 5 contains ten years, we have computed $\bar{J}^t$ by taking a simple average of average family size over the age groups $\tau \geqslant t$. Thus the value of $\bar{J}^t$ at the beginning of age group t is computed by the formula:[2]

$$\frac{\sum_{\tau=0}^{6-t} J_{t+\tau}}{6-t+1}.$$

However, since we are interested in the value of $\bar{J}^t$ for the age group as a whole, it must be 'centred' which we have done with the following formula:

$$\bar{J}^t = \frac{\sum_{\tau=0}^{6-t} J_{t+\tau} + \sum_{\tau=1}^{6-t} J_{t+\tau}}{(6-t)+(6-t+1)}. \tag{3.17}$$

---

[1] The above propositions are valid only when the following conditions are roughly satisfied. Let us write the coefficients of (3.14) for the ith household of age t as:

$$(H_t G_t v_0^{(t)}/L_t)^{(i)} = (\overline{H_t G_t v_0^{(t)}/L_t}) + \epsilon_0^{(i)}$$

$$(H_t G_t v_1^{(t)}/L_t)^{(i)} = (\overline{H_t G_t v_1^{(t)}/L_t}) + \epsilon_1^{(i)}$$

$$(G_t/L_t)^{(i)} = (\overline{G_t/L_t}) + \epsilon_2^{(i)}$$

where $\left(\overline{\frac{H_t G_t v_0^{(t)}}{L_t}}\right)$, $\left(\overline{\frac{H_t G_t v_1^{(t)}}{L_t}}\right)$ and $\overline{(G_t/L_t)}$ are the mean values of each coefficient over all households of age t. We then require that ϵ_0, ϵ_1 and ϵ_2 be distributed independently of each other and also of the variables y and A. See Theil [10], especially chapter 2, for more exact conditions for these aggregations.

[2] In this and following formulae the age index t refers to Fisher's age groups rather than to actual ages of the individual household.

In order to estimate the coefficient $Q_A^{(t)}$, we can now insert the values of J_t and $\bar{J}^t$ so computed into the equation (3.15b) and divide by

$$L_t = 10(6-t+1/2).$$

In column (2) of Table 3.1 we show the result of this computation, using for α the value of .5.[1] The figures of this column as well as all the remaining columns of this table are index numbers where the average value of the entries for the age groups 2–5 inclusive are taken as 100. It need hardly be stressed that this procedure is only meant to give us a very rough notion of the behaviour of the coefficient of net worth over life.[2] In particular, the numerical value we have assumed for α is quite arbitrary, although fairly large variations in α could not affect the picture substantially. Furthermore, if we took the estate motive into account, the rise would be less pronounced, especially at the end. On the whole, while it is certainly true that variations in the assumptions will somewhat modify the results, it is clear, from the figures of column (2), that, if our model is correct, the marginal propensity to consume with respect to net worth should tend to rise appreciably with age, and reach its peak in the last age group.

In order to obtain an idea of the behaviour of the parameters Q_0 and Q_y, we can proceed along the same lines we have followed in estimating Q_A. Comparing equations (3.14) and (3.16) and remembering the definitions (3.15), we can write

$$Q_y^{(t)} \simeq v_1^{(t)}\left(\frac{\bar{Y}_t}{\bar{p}_t}\right)Q_A^{(t)}. \tag{3.18}$$

We first note that $\bar{p}_t$ is equal to $\bar{y}_t$. To estimate $\bar{Y}_t$, we proceed as in the case of $\bar{J}_t$, that is, we cumulate the average income of age groups τ larger than or equal to t. Since we are interested in the behaviour of households who do not retire until the age of 65, for the age groups 1 to 5, we used the estimate of average income obtained from Fisher's data after eliminating the retired and unoccupied households. For age group 6, in principle, we should include only that part of receipts that represent earned income. The remaining part of receipts, representing non-earned income, *e.g.*, receipts from pension funds and annuities should not be included in $\bar{Y}$ but reflected through A. Lacking any precise information concerning this breakdown, we have estimated earned income to represent 40 per cent of the average income of the age group 6.[3]

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[1] For the age group 1, a slight adjustment of the formula is necessary, because it contains only 7 years. It may be noted that the estimate for the first group is particularly crude because not all the households are formed at age 18. This difficulty however does not affect the estimates for later age groups, for they do not involve information pertaining to any earlier age groups.

[2] For instance, the averaging process just described is not strictly equivalent to computing $\overline{G_t/L_t}$ for all households, except under a very special circumstance.

[3] This estimate is obtained by assuming that the aggregate amount of earned income of group 6 is approximately equal to the aggregate earnings of non-retired households in this age group, earned receipts of retired households offsetting unearned receipts of non-retired households. Since the average receipts of non-retired households computed from Fisher is about £400, and the proportion of the non-retired households in this age group is about 20 per cent (see Appendix C in Fisher), the average earned receipts come out to be roughly £80. This figure represents 40 per cent of the average income of the age group 6, namely, £200.

TABLE 3.1

Expected Behaviour of Marginal Propensities to Consume and that of Fisher's Estimates[1]

(1)	(2)[4],[2]	(3)[5],[2]	(4)[6],[2]	(5)[7],[2]	(6)[8],[2]	(7)[9],[2]	(8)[3]	(9)[3]	(10)[3]	(11)[3]	(12)[3]	(1)
Age group	$\overline{(G_t/L_t)}$	$\overline{H_t}$	$\left(\overline{\frac{G_tH_t}{L_t}}\right)$	$\left(\overline{\frac{G_tH_t^*}{L_t}}\right)$	$\left(\overline{f_1\frac{G_tH_t}{L_t}+f_2\frac{G_tH_t^*}{L_t}}\right)$	$\left(\overline{f_1\frac{G_tY_t}{L_t}+f_2\frac{G_tY_t^*}{L_t}}\right)$	Q_y	Q_A	Q_0	q_y	q_A	*Age group*
1	26	393	129	119	118	61	116	263	3	121	119	1
2	52	183	120	111	111	105	98	160	122	102	169	2
3	85	114	120	114	114	126	100	76	107	101	48	3
4	107	71	94	93	93	99	96	75	133	99	92	4
5	157	33	66	81	82	70	106	89	38	97	90	5
6	435	24	130	112	118	50	120	116	9	123	117	6

[1] All figures shown are indices where the average value for age groups 2–5 inclusive is taken as 100.

[2] Data utilized in computing these values are taken from [1], [5], and [6].

[3] Based on Fisher. See his Appendix A, Tables VI and VIII. See also Text, pp. 119, 120, 122 and equations (3.16) and (3.20).

[4] The expected behaviour of Q_A and q_A. See text, pp. 115, 117.

[5] See Text, pp. 115, 117. [6] See Text, pp. 115, 117. [7] See Text, p. 117.

[8] The expected behaviour of Q_y and q_y except for the factor v_1. See pp. 115, 117.

[9] The expected behaviour of Q_0 except for the factor v_0. See p. 118.

The estimates of $\overline{Y_t/P_t}$ so computed is shown in column (3). By multiplying this column by Q_A given in column (2) we obtain an estimate of the behaviour of Q_y up to the proportionality factor $v_1^{(t)}$.

Our model does not yield any direct information on the variation over life of v_1, which depends essentially on the short-run variability of income.[1] However, we feel that the index given in column (4) represents at least a rough indication of the behaviour pattern of Q_y over life. This is because we have no reason to suppose that, at least in age groups 2 to 5, short-run income variability changes sufficiently with age as to upset the pattern of Col. (4). On the other hand, the actual level of Q_y should vary for groups with different income variabilities within a given age group. For instance, we would expect it to be lower for the enterpreneur group, as compared to the average.

It should be noted that the computations of columns (3) and (4) are meaningful only for households who do not plan to retire until age 65 or thereabout, and to the extent that the net worth A is defined to include accumulated reserves into pension funds and other forms of insurance schemes, that will serve as the source of unearned income after retirement. If the estimates of net worth do not reflect these reserves, as in the case of the British Survey, it would appear preferable to include unearned as well as earned income for the age group 6 in computing $\bar{Y}$. The value of $\overline{Y_t/p_t} \times Q_A^{(t)}$ obtained under this alternative methods of estimating $\bar{Y}$ is shown in column (5). Actually, since the accumulation of reserves occurs more or less continuously over life, it is clear that the figures of column (4) become gradually less meaningful and those of column (5) more meaningful as we move from age 1 to 5. Presumably, the best approximation would require a weighted average of these two columns with weights shifted increasingly to column (5). In column (6) we exhibit such an average based on fairly crude guesses as to the appropriate pattern of weights.[2] The general picture that emerges is, at least qualitatively, unchanged.

We may conclude, therefore, that, neglecting variations in v_1, the marginal propensity to consume with respect to income should tend to fall with age up to group 5 (except possibly between ages 2 and 3) and then to rise in age group 6.[3] In the above computation, we have ignored a possible effect of secular growth of real income. It is conceivable that, because of such growth, a household currently of age t might expect to receive an income higher than that received by a household currently of age $t+\tau$ when the former reaches the latter's age. The adjustment for this effect of secular growth—

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[1] See formula (3.13b) and footnote * on page 112.

[2] On the assumption that the reserves are accumulated at a constant rate over life, the following weights, f_1, are used for column (4) for the ages 1 to 5 respectively: .93, .75, .53, .32, .11. The weights for column (5) f_2, of course, are the complements of the above weights.

[3] In informal conversations with staff members of Survey Research Center, we were told that the data collected by them suggest that short term variability of income tends to decrease with age. Unfortunately, this information is neither quantitative nor necessarily applicable to the British data. If this information is correct, the fall of Q_y with age would be less pronounced than indicated in Column (6). It seems to us, however, that the fall of Q_y is not likely to disappear as the result of this movement of v_1.

the desirability of which is by no means obvious—would tend to raise the coefficients for the earlier age groups relative to those of later age groups. Thus, in any case, this adjustment would not change materially the systematic downward drift of the income coefficients over life.

The remaining coefficient, Q_0, remembering again definitions (3.13) and (3.15) reduces to

$$Q_0^{(t)} \simeq v_0'^{(t)} \dot{\bar{y}}_t \left(\frac{\bar{Y}_t}{P_t}\right) Q_A^{(t)} \simeq v_0'^{(t)} \bar{Y}_t Q_A^{(t)} \tag{3.19}$$

The methods of estimating the last two factors have already been discussed. Two alternative estimates of their products corresponding to the alternative estimates of $\bar{Y}_t$ can be obtained. The index shown in column (7) is an average of these two estimates with weights identical to those used in column (6). It may be noted that two alternative estimates underlying the average do not differ appreciably. According to column (7), the product tends to reach its peak around age 3, and falls monotonically thereafter. The constant term Q_0 involves the additional factor $v_0'^{(t)} = (1 - r^2_{y_t p_{t-1}})(1 - w_1)$ which is a decreasing function of the short term variability of income. As indicated in connection with v_1, we have no reason to believe that this factor varies greatly over age groups, so that the figures of column (7) can be used as an indication of the behaviour of $Q_0^{(t)}$ as a function of t.

We now have a general picture of the behaviour of the coefficients of equation (3.16) as a function of age implied by our model. This picture should be compared with the least squares estimates of these coefficients. Unfortunately, Fisher's regression equations are far from adequate for this test, even if we are willing to put aside certain serious doubts concerning the adequacy of the basic data, discussed in the Sections 1 and 2.

As Fisher is perfectly aware, his definition of S is not a satisfactory measure of saving in our model. Far more serious is Fisher's decision to use liquid assets as a proxy for net worth, especially since he was in the rather unique position of having fairly complete information on net worth. To be sure, the information on this variable is probably subject to considerable error. But we cannot see any grounds for arguing that liquid assets can possibly provide a better measure of net worth than a direct estimate.

Since the M-B model has nothing to say concerning the structure of asset holdings, we can only derive from it implications concerning the marginal propensity to consume with respect to net worth. Because liquid assets is a variable proportion of net worth, and it is a relatively small proportion especially for the older age group, there is no reason to expect the marginal propensity to consume with respect to liquid assets to behave in accordance with the figures in Column (2). Moreover, the substitution of liquid assets for net worth will also systematically bias the coefficient of income and the constant term. The fall with age of the marginal propensity to consume with respect to income observed in column (6) is due essentially to the fact that as the household grows older, current and prospective income from employment, Y, represents a decreasing proportion of total resources $Y+A$, since Y falls while A rises with age. Hence, the pattern exhibited by columns (6) and (7) are valid only if we succeed in effectively controlling net worth in its broadest definition. There are strong grounds for doubting that merely to control liquid assets will serve this purpose. In fact, it is likely that the substitution of liquid assets for net worth produces an upward bias in the coefficient of income, growing in size with age.[1]

It must be painfully clear by now that Fisher's test can throw very little light on the usefulness of our model. Nevertheless, it is worth while to take a careful look at Fisher's results since, for all their limitations, they are the only available ones at this time. The tests which are relevant at this point are those involving regressions of savings, S, on y and L. It must be remembered, however, that the figures in Table 3.1 refer to the marginal propensities to consume, while his coefficients are marginal propensities to save.[2] Hence, Fisher's coefficients are converted into marginal propensities to consume before they are transformed into the index form for presentation in Table 3.1.

Ideally, we should look at the results relating to all occupations except retired and unoccupied, for age groups 1 to 5, and to all households for age group 6, since these are the households to which our model refers. Unfortunately, the regression of S on y and L is not given in Table VIII of Fisher's paper (relating to all occupations except for retired and unoccupied). We must be satisfied by the results of Table VI, which relates to all occupations. The income coefficients from Table VI are shown in column (8) of Table 3.1. The observed behaviour is roughly consistent with our model except for age group 5, where the marginal propensity rises prematurely. However, as will be presently shown, this may well be due to the inclusion in this class of a sizeable proportion of retired households. The coefficient of liquid assets and the constant are exhibited in columns (9) and (10) of Table 3.1 respectively. The reader can verify that the asset coefficient does rise with age but only starting with age 3; and that the constant term, in spite of some irregularities, does exhibit the expected parabolic pattern over life.

Fisher provides us with similar estimates for various professional classifications. We shall not stop to analyze these coefficients in any detail. In the first place, it would be necessary to recompute the relevant columns of Table 3.1 on the basis of the life cycle of income and family size appropriate to each occupation. Second, and much more important, the samples on which

[1] Estimates of net worth and liquid assets by income classes given in [9], Tables VII and IX, suggests that there may be an appreciable correlation between income and net worth given liquid assets. If this is so, in the multiple regression, the coefficient of income will absorb a part of the weight which properly belongs to net worth. Since the coefficient of net worth is supposed to rise with age, the over-estimate of the income coefficient will itself rise with age.

[2] By marginal propensities to consume (or save) with respect to a variable, say y, we simple mean the coefficient of y in the regression equation estimated from the cross section data, and not the extent by which the consumption of an individual household would rise per unit change of its current income. The latter concept, which is closer to Keynes's original definition, would be impossible to get at from the type of data we are dealing with, and does not concern us here. See [8] on this point.

his estimates are based are too small to warrant much attention. The only significant exception is in the case of manual workers where the samples in general contain more than 100 observations. However, in this case, the coefficients obtained are apparently not very meaningful. Since we expect the income variability in this group to be relatively small, we should be prepared to find the marginal propensity to save to be relatively small. However, we are not willing to accept Fisher's results that three of the six coefficients are negative as a valid description of the population at large, at least pending much further evidence. Furthermore, half of the coefficients are smaller than the standard errors. This result indicates, in our opinion, that even this sample is not large enough to yield reliable estimates, and it is not worth while to pay any attention to the differences among the coefficients of different age groups. We may note in passing that, in the case of the self employed, the marginal propensity to save with respect to income and the constant term tend to be far greater than for the population as a whole. However, the sample in this instance is so small, considering the heterogeneity of this group, that we are not inclined to attach much significance to this apparently favourable result.

Fisher has also provided us with alternative estimates in which the dependent variable S' is defined to include the gross investment in durables. We believe that these tests are even less relevant than those using S as the dependent variable. There are several reasons for this conclusion. In the first place, while it is quite true that in our model the purchase of durables is not consumption, it is not true that such purchases constitute saving unless we subtract the depreciation of durables from the saving thus computed. Note in particular that, while according to our model saving should be roughly proportional to (permanent) income, there is no reason to believe that this should be true of S'. Indeed, the desired stock of durables, and hence the expenditure necessary to maintain the stock, may rise more than proportionately to income, at least in certain income ranges.

Even the difficulties arising from these complications might not have been fatal if Fisher had used the correct definition of net worth instead of liquid assets. The use of liquid assets is particularly troublesome when the dependent variable is the gross saving, S'. This is because liquid assets is the portion of net worth which is likely to be systematically related to the purchases of durables. In fact, as some authors have pointed out, there is a tendency on the part of the household to accumulate liquid assets immediately prior to a large purchase of durables. Hence, while saving properly defined should tend to fall with net worth, it is not only possible but probable that S' tends to rise with liquid assets. It is, therefore, not surprising to find in Fisher's Table VI that when the dependent variable is S', the coefficient of liquid assets is positive in three of the six age groups. If, instead of liquid assets, Fisher had used another sizeable portion of net worth, *i.e.*, the stock of durable goods, it is possible that the coefficient of assets would be biased in the opposite direction. For this reason, we will pay no further attention to the test involving S'.

We now proceed to examine the second set of Fisher's tests in which family size is introduced as an additional independent variable in linear form. The relevant regression equation now takes the form

$$c_t = q_0^{(t)} + q_y^{(t)} y + q_A^{(t)} A + q_J^{(t)} J + u_t. \tag{3.20}$$

In order to see the implications of our model for the behaviour of the coefficient $q_J^{(t)}$ of the new variable, J, and for the effects of the introduction of J in the regression equation on the behaviour of other coefficients, we must examine the correlation between G and J for a given age group. Let

$$G_t = j_0^{(t)} + j_1^{(t)} J + z_t \tag{3.21}$$

be the regression equation of G on J for the age group t. Unfortunately, we can say very little about the values of j_0 and j_1. We seen from the definition of G given by (3.15b), that as current family size, J, rises, the value of the numerator of G increases. But $\bar{J}$ in the denominator will tend to rise also, increasing the value of the denominator. Since we have no information about the relative movements of J and $\bar{J}$, we cannot make any statement about the movement of G as a function of J, and hence, the values of j_0 and j_1.

For older age groups, we would rather expect j_1 to be positive because small families are not likely to grow larger, while large families are likely to become smaller. But, for younger age groups, we would hesitate to venture a guess even about the sign of j_1. In any event, j_1 is likely to be rather small in absolute value. This in turn suggests that $j_0^{(t)} = \bar{G}_t - j_1^{(t)} \bar{J}_t$ must be close to $\bar{G}_t$.

Let us now substitute (3.21) into (3.14) obtaining

$$\begin{aligned} c_t &= \frac{H_t}{L_t}(j_0^{(t)} + j_1^{(t)} J)(v_0^{(t)} + v_1^{(t)} y) + \frac{1}{L_t}(j_0^{(t)} + j_1^{(t)} J)A \\ &= j_0^{(t)} v_0^{(t)} \frac{H_t}{L_t} + j_0^{(t)} v_1^{(t)} \frac{H_t}{L_t} y + \frac{1}{L_t} j_0^{(t)} A + j_1^{(t)} v_0^{(t)} \frac{H_t}{L_t} J \\ &\quad + j_1^{(t)} v_1^{(t)} \frac{H_t}{L_t} yJ + \frac{1}{L_t} j_1^{(t)} JA. \end{aligned} \tag{3.22}$$

Thus, we see that, even if the linear approximation (3.21) is accepted, the resulting regression equation is of the form

$$c_t = q_y'^{(t)} + q_y'^{(t)} y + q_A'^{(t)} A + q_J'^{(t)} J + q_{yJ}'^{(t)} yJ + q_{AJ}'^{(t)} AJ + u_t \tag{3.23}$$

rather than (3.20). If the regression equation (3.23) were estimated, we could readily infer from equation (3.22) the behaviour of the coefficients in equation (3.23), as was done in the case of equations (3.14) and (3.16). However, Fisher works with a regression equation of the form (3.20) rather than (3.23), and we must make further approximations. Expanding product terms in a Taylor series around the means and remembering the definitions (3.13), (3.15), and (3.21), we finally get

$$c_t = \left[v_0^{(t)} \frac{H_t}{L_t} \bar{G}_t - j_1^{(t)} \bar{J}_t \frac{1}{L_t}(\bar{Y}_t + \bar{A}_t)\right] + v_t^{(t)} \frac{H_t \bar{G}_t}{L_t} y + \frac{1}{L_t} \bar{G}_t A + j_1^{(t)} \frac{1}{L_t}(\bar{Y}_t + \bar{A}_t) J \tag{3.24}$$

As explained in detail earlier in this section, it is the average values of the coefficients in the equation (3.23) over all the households in the age group t which should be compared with the corresponding coefficients of equation (3.20). Hence, we can conclude that the expected behaviour of $q_y^{(t)}$ and $q_A^{(t)}$ is exactly the same as that of $Q_y^{(t)}$ and $Q_A^{(t)}$ of equation (3.16) and for these two coefficients, the appropriate columns of the Table 3.1 can be used for comparison.

The constant term differs from that in equation (3.14) only by the subtraction of the second term. If $j_1^{(t)}$ is very small in the absolute value, as we are inclined to suppose, $q_0^{(t)}$ must be fairly close to $Q_0^{(t)}$, and should move in the manner shown in column (7). As for the coefficient $q_J^{(t)}$, we see that it is the product of two factors, j_1 and $(\bar{Y}+\bar{A})/L_t$. The second factor of this product could be estimated from Survey data as we have done for the remaining coefficients in Table 3.1. It can be verified that this quantity has a very flat parabolic pattern reaching its peak at age group 2. It is not worth while, however, to exhibit the expected behaviour of $\frac{1}{L_t}(\bar{Y}+\bar{A})$, since the uncertainty surrounding the behaviour of j_1 makes it impossible to infer the behaviour of $q_J^{(t)}$ from that of $\frac{1}{L_t}(\bar{Y}+\bar{A})$.

We can now compare the expected behaviour of the coefficients of (3.20), with the estimates obtained by Fisher using S as the dependent variable, and y, L, and J as the independent variables. In so doing it should be remembered that all the qualifications set forth in connection with the earlier tests are equally pertinent here.

In column (11) of Table 3.1, we exhibit the behaviour of the coefficient of income as given by Fisher in Table VIII for age groups 1–5 and in Table VI for age group 6. The general behaviour of this coefficient is substantially in agreement with the pattern suggested by column (6). It is particularly worth noting that the coefficient for age group 5 which is now available for the cross section excluding retired households is lower than for any other age group, as expected. At the same time it should be recognized that the observed variation with age is more than that implied by our model from the age group 1 to 2, and less than that implied by our model between the age group 3 and 5. Pending further tests, we may conjecture that this may be a consequence of the use of liquid assets in the place of net worth, as indicated in the preceding discussion.

The coefficient of liquid assets, given in column (12) again follows the direction appropriate for the coefficient of net worth only after age group 3.

The coefficients of family size in Fisher's estimates behave rather erratically. This does not surprise us, since we have indicated that no definite *a priori* statement can be made about this coefficient. The very tentative inference we have made about the sign of the coefficient of J for the later age groups appears to be mildly supported by Fisher's results.

There remain to make a few remarks concerning the tests by occupational classification. Our earlier comments to the effect that the results of these tests are almost impossible to assess apply substantially again, except that for the present test we have results available for the combined group excluding retired and self-employed, given in Table IX. Here the behaviour of the asset coefficient is completely erratic. On the other hand, the coefficient of income moves in close conformity to our model for all age groups except for the age group 5, where there is a sharp drop which is definitely not accounted for by our model.

Conclusion

It should be clear by now that the conclusions which emerge from our labour are painfully meagre.

As shown in Part I, the serious difficulties that we encounter in reconciling savings with net worth estimates unavoidably raise some questions as to the adequacy of the basic data, and the reliability of the tests based upon time. As shown in Part II, there are even more reasons to doubt the representativeness of Fisher's sample after his monitoring, at least for the purpose of analyzing the effects of age. Finally, in Part III, we have shown that Fisher's correlation tests are largely irrelevant for the purpose of testing the M-B model.

Thus, the most we can say is that in terms of the behaviour of net worth and of the average and marginal propensities to save, the model performs reasonably well, though not uniformly, in a set of tests which is largely inadequate and/or irrelevant. This outcome does not provide much ground for either rejoicing or despairing. For, where the model fails, we can still blame the results on the inadequacies of the tests, while by the same token, successful instances might not survive more relevant tests.

This does not mean that we feel Fisher's work or this lengthy comment is useless. On the contrary, we are very grateful to Fisher for his pathbreaking attempt at systematic testing. Furthermore, his effort has in turn provided stimulation for us to set down in an orderly fashion some of the major quantitative implications of our model. The assumptions utilized in this paper for this purpose are far more adequate than the expository assumptions made in the original paper of Modigliani and Brumberg [8] even though further improvements are certainly possible.

Our task in this comment is at least partially accomplished if we have succeeded in convincing the reader of the desirability of performing further tests. We think it is one of the merits of our model that it gives a person with the right kind of data so many opportunities to refute it. We, therefore, look forward to seeing the results of such future tests, with high hopes even if with some misgivings.

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SAVINGS AND THE BALANCE SHEET

By Milton Friedman

Fisher's thoughtful, thorough, and imaginative examination of the 1953 Savings Survey data in the light of the related theories recently suggested by Modigliani and Brumberg and by me raises some important questions about both the data and the theories. It is encouraging that his data do not contradict the central idea in these theories. It is somewhat discouraging that the agreement is so loose and that there are so many points at which the evidence seems inadequate either to choose between the two theories or between the two of them and the theories they are designed to supplant.

Part of the reason for the loose agreement is, I believe, to be found in deficiencies in the data Fisher analyzes (see especially point 3 below). But part of the reason is to be found also in what seems to me the most stimulating feature of the paper—Fisher's attempt to go beyond the original formulations of the theories. For he is here not testing clearly stated implications of the theories but rather extending and revising them. To speak only of my own formulation, I pay little more than lip-service to the role of wealth or assets. This is a serious limitation and I feel that the next major step in the development of the theory will be its extension to cover the structure of assets—the disposition of positive savings or the means used to finance negative savings. Fisher's examination of the role of liquid assets is an important step in this direction and, while he has not reached the goal, I suspect that whoever seeks to go farther will find Fisher's formulation an indispensable stepping stone.

The following comments on Fisher's paper deal mostly with issues involving my hypothesis. They are designated by page numbers in Fisher's article and are in that order, except for the final comment which is more general.

1. Page 229. Fisher states, ' Friedman's hypothesis is not specific about the variation of the average propensity to save with age and family size '. This statement is correct if ' average propensity to save ' is taken to mean the *permanent* propensity, that is, the ratio of mean permanent savings to mean permanent income. It is not correct with respect to age for the *measured* propensity. On the permanent income hypothesis as I formulated it, one might expect mean measured income to be less than mean permanent income at relatively low and relatively high ages and greater at intermediate ages. This in turn implies a relatively low ratio of measured savings to measured income for the extreme age classes and a relatively high ratio for the intermediate ages. In the monograph that Fisher refers to,[1] this implication is tested with both American data and data from the U.K. Savings Survey. Both conform to it (see further comments in point 3 below).

2. Pages 232–235. Fisher's explanation of the relatively low savings

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[1] M. Friedman, *A Theory of the Consumption Function*, Princeton University Press for the National Bureau of Economic Research (in press).

ratio of self-employed in age group 4 as a reflection of the abnormally high liquid assets ratio seems eminently reasonable. But he does not give a satisfactory explanation of the high liquid assets ratio itself. In the context of the table, this ratio looks decidedly out of line. Though based on 40 observations, so sampling fluctuations should be moderate, it is more than triple the ratios in the neighbouring age classes of self-employed, and higher than any other ratios in Table 2.1 except for the two oldest age groups of retired and unoccupied—who might be expected to have high ratios of liquid assets to income. The high ratio looks very much like a statistical quirk of some kind, and it therefore seems dubious that much economic significance should be attached to it.

3. Page 239. This table seems to me to deserve a fuller analysis than Fisher has given to it. Taken at face value, it is highly unfavourable to the permanent income hypothesis. On that hypothesis, P_Y must be less than unity. Yet of the 23 estimated values in the table for separate age and occupation groups, 10 are equal to or greater than unity for the broader definition of consumption [definition (*a*) including durables] and 3 even for the narrower definition [definition (*b*) excluding durables]. And the ten classes with P_Y on definition (*a*) greater than unity contain 906 of the 1554 observations used in constructing the table. Unless this result can be explained satisfactorily, it must be regarded as a serious contradiction of the hypothesis.

Two explanations can be ruled out. One is that concept (*a*) is inappropriate and hence only the values based on concept (*b*) should be taken seriously. If concept (*a*) is too broad, concept (*b*) is too narrow. More important, I have used a concept like (*a*) in most other applications that I have made of the permanent income hypothesis and have not hitherto found any contradictions so sweeping as this. I cannot very well accept (*a*) when it works and reject it when it does not. The second is that the assumptions used in computing P_Y are not acceptable. These values are computed on the assumption that mean transitory consumption and mean transitory income are zero within each cell. I see no objection to this assumption for consumption. As noted in point 1, however, it is not acceptable for income. Mean transitory income is presumably negative in the lower and higher age classes and positive in the intermediate ones. To judge from my earlier analysis referred to in point 1, it is negative in age groups 1, 5, and 6; positive, in age groups 2, 3, and 4. But allowance for this makes matters worse rather than better. Estimates computed as in this table are under-estimates if the mean transitory income is negative, over-estimates if it is positive. Of the 10 estimates of P_Y on concept (*a*) that exceed unity, seven are for age groups 1, 5, and 6, and hence are if anything too low.

The correct explanation is, I believe, to be sought elsewhere. An estimated value of P_Y greater than unity means that the computed marginal propensity to consume exceeds the computed average propensity. Of the 10 values on concept (*a*) that exceed unity, eight are for classes for which the computed marginal propensity not only exceeds the average propensity but also exceeds unity, *i.e.*, the marginal propensity to save is negative—and these are the only classes for which this is true. Now a negative marginal propensity to save contradicts not only the permanent income hypothesis but every hypothesis I know of that has been seriously considered; it cannot therefore be regarded as contradicting the permanent income hypothesis by comparison with any other. Furthermore, it contradicts a host of other empirical evidence—the marginal propensity to save as computed from cross-section budget study data is almost invariably positive. The clear implication is that there is something wrong with these data.

In the analysis mentioned earlier of the relation of savings to age, I found that although both the U.K. and U.S. data showed the general pattern expected, the U.K. data were much more irregular and erratic than the U.S. I conjectured that the explanation might be the definition of the income unit used in the U.K. study, which differs in one important respect from that used in the U.S. study. The observed negative marginal propensities to save may be another manifestation of the same thing. In the Savings Survey, a single person of 18 years or more who is living with other members of his family is counted as a separate income unit together with any dependent children he has, unless his income is less than £50. Now, such secondary income units, as I shall call them for convenience, might be expected to have quite different savings from primary units alike in respect of age, occupation, and measured income. Typically, the measured income of the secondary spending unit is not its whole income; it is likely to be receiving income in kind in the form particularly of housing and food from the primary unit, and, indeed, the availability of such income may be a major reason why the two units live together. In consequence, one might expect its savings to be larger (in algebraic value) than the savings of its primary counterpart.

Secondary spending units are particularly likely to be found in the younger and older age groups, and it is noteworthy that five of the eight negative marginal propensities to save are for age classes 1 and 6, and two more for age classes 2 and 5. The final one is for age group 4 for manual workers. This one is —.001, which is the smallest in absolute value of any of the negative marginal propensities and only 1/45 of its standard error, so it may well reflect simply sampling error. My conjecture is that the other negative marginal propensities arise from a mixture in the respective classes of primary and secondary units, with the secondary units concentrated toward the lower part of the income scale, and the primary units toward the top, and with the secondary units having larger savings on the average than the primary units. Under these circumstances, the computed marginal propensity to save could be negative for the two types of units combined, even though it were positive for each type of unit separately. This conjecture can presumably be tested by separating out the primary and secondary units and seeing whether each separately yields a positive marginal propensity to save. It is important that this be done since until some explanation is found for the negative marginal propensities to save, the data yielding them must be regarded as highly questionable if not worthless not only for this analysis but for any other. If the conjecture should be confirmed, it would mean that classification by type of income unit (primary, no secondary present; primary, one or more secondary units present; secondary unit) would contribute greatly to an analysis of the data.

If we leave to one side the eight cells in which the marginal propensity to save is negative, there remain only two in which the computed value of P_Y is not less than unity, and in both of these the value is 1.00—group 3 for manual and 5 for clerical and sales. In the first, mean transitory income is presumably positive, so the estimated value is an over-estimate of the correct value; in the second, a mixture of primary and secondary units may well be present and have lowered the marginal propensity to save though not by enough to make it negative. In any event, these two borderline cases do not constitute a serious contradiction when balanced against the 13 estimated values of P_Y that are less than unity.

4. Page 240. Fisher tends to interpret P_Y as an index of uncertainty or variability of income prospects and to regard the roughly similar estimated values of P_Y for managers and self-employed as implying similar degrees of income variability. This result troubles him since one would clearly expect variability to be greater for self-employed. Now there is a relation between P_Y and the variability of income prospects but it is not so direct as Fisher's discussion suggests and, in particular, the same value of P_Y can correspond to quite different degrees of variability.

Let σ_P be the standard deviation of permanent income, σ_T that of the transitory component of income, and σ that of total income, and let $\overline{Y}_P$ be mean permanent income. The degree of income variability, or uncertainty of income prospects, is measured by $\sigma_T/\overline{Y}_P$, or by some similar statistic, that is, by the percentage dispersion of transitory components. P_Y is defined by $\frac{\sigma_P^2}{\sigma^2}$, or, on the assumption that permanent and transitory components are uncorrelated, by $1 - \frac{\sigma_T^2}{\sigma^2}$. Two groups can therefore have the same value of P_Y and the same mean income yet different values of σ_T, provided σ_P varies to compensate. $(1-P_Y)$ measures the *fraction* of the total variation in income contributed by the transitory component, not the absolute amount.

The similar and relatively low values of P_Y computed by Fisher for managers and self-employed do, as he says, explain, on the permanent income hypothesis, the similar and relatively high values of the marginal propensity to save. But they do not necessarily imply roughly equal uncertainty of income. Some American data that I have analyzed show a decidedly greater percentage variability of permanent income $(\sigma_P/\overline{Y}_P)$ for independent business and professional families than for salaried business and professional. If this is true also for the U.K., the common values of P_Y imply a decidedly larger uncertainty of income ($\sigma_T/\bar{Y}_P$) for self-employed than for managers, the result one would expect.[1]

5. Page 243 *et seq. The rôle of liquid assets.* As noted earlier, this seems to me the most novel and interesting part of this paper. Accordingly, I shall in this case go beyond textual criticism and seek to carry Fisher's analysis a bit farther.

Suppose we compute, as Fisher and others have done, multiple correlation equations relating measured consumption or measured savings to measured income and measured liquid assets. The results are almost invariably, as they are in this paper, that the partial marginal propensity to consume out of measured liquid assets is positive; that is, for given measured income, measured consumption is higher, which is to say, measured savings are lower, the higher is measured liquid assets. In addition, it is generally found, as in this paper, that there is an interaction between the effect of measured income and measured liquid assets, so that the marginal propensity to consume out of measured income is lower, the higher are measured liquid assets. The problem is to explain these results.

On the permanent income hypothesis, the liquid asset variable plays a dual role. On the one hand, it is, or may be, a proxy variable, and this itself in two ways; on the other it is, or may be, a variable in its own right. Presumably, other things the same, desired liquid assets are higher, the higher is permanent income, and this presumption is confirmed by a wide range of statistical evidence, including that in Fisher's paper (*e.g.* Table 1.1). Presumably, further, actual liquid assets are positively correlated with desired liquid assets. Suppose, now, we select a group of income units with the same *measured* income. These units will not all have the same permanent income or the same liquid assets. The higher the permanent income, the higher the liquid assets and conversely. For given measured income, therefore, liquid assets can serve as a proxy variable for permanent income. Even if consumption depended only on permanent income and for a given permanent income were entirely independent of the level of liquid assets, there would be a positive correlation for a given measured income class between consumption and liquid assets because of the positive correlation between liquid assets and permanent income. As Fisher points out, this effect is in the same direction as that observed, so that the observed results cannot without further analysis be regarded as evidence that the liquid asset variable is important in its own right.

There is a second way in which liquid assets may serve as a proxy variable. For a given permanent income, one would expect desired liquid assets to be higher, the more uncertain the income prospects. But in general, though for the reasons given in the preceding point not necessarily, P_Y will be less, the more uncertain the income prospects. And the lower P_Y, the lower, on the permanent income hypothesis, is the computed marginal propensity to consume out of measured income. In consequence, a relatively high level of liquid assets might be expected to be associated statistically—as in fact it is—with a relatively low marginal propensity to consume out of measured income.

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[1] A direct estimate of $\sigma_T/\bar{Y}_P$ can be derived, if one can take the mean transitory component of income to be zero, by multiplying the estimated σ^2 by the estimated $(1-P_Y)$, taking the square root and dividing by mean income. This is a much better basis than P_Y itself for judging the uncertainty of income prospects. I have not been able to make the calculation since Fisher does not give the standard deviations of income.

These proxy effects of liquid assets are in the same direction as the observed effects and so could wholly account for them. The interesting question is whether they do, or whether liquid assets have an effect in their own right. It is not clear what should be expected on theoretical grounds. As Fisher explains, one would expect total assets higher than the desired level to raise consumption and conversely. It is much less clear that this will be so for liquid assets alone, since deviations of liquid assets from the desired level may not reflect deviations of total assets from the desired level and can be adjusted by altering the composition of assets rather than by changing consumption. Any relation between liquid assets and consumption could therefore be expected to be weaker than between total assets and consumption. If there were any effect, its statistical counterpart in the multiple regression equation would be in the same direction as the proxy effects just considered. In consequence, the sign of the coefficient of the liquid asset variable tells us nothing about whether this variable has an effect in its own right; for this, we must analyze the size of the term, which is a much more difficult task. This is the problem Fisher tackles on pp. 242–43, though without being able to reach a definite answer.

The best way, I believe, to test whether the liquid asset variable exerts an influence in its own right is more direct than the method Fisher adopts. Take a group of units that can be expected to have the same ratio of permanent consumption to permanent income so far as variables other than liquid assets are concerned. Classify them by liquid assets. For each liquid asset class, compute mean consumption and mean income, and the ratio of the two. If the liquid assets variable has an influence in its own right, the resulting average propensities to consume should rise with liquid assets. If they do not, the implication is that the observed effect of liquid assets is to be explained entirely by its role as a proxy variable.[1]

Fisher proceeds more indirectly. He constructs a model designed to provide an estimate of γ, which measures the effect of the liquid asset variable in its own right, and to reveal any bias in the observed partial regression coefficient c as an estimate of γ. His model seems to me correct in its main outlines. Unfortunately its results are inconclusive. The estimates of γ are small and may well not differ significantly from zero, but one cannot be sure.

The basic relation in Fisher's model is

$$C_P = \beta\, Y_P + \gamma\, L_T, \tag{1}$$

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[1] The observed ratios will tend to have a zero correlation with liquid assets if liquid assets are uncorrelated with the transitory component of income. For reasons pointed out below, there is some reason to expect a mild positive correlation between initial liquid assets and the transitory components of income. The effect of this would be to produce a negative correlation between the average propensities to consume and liquid assets, which is why the test is stated as it is in the text.

where C_P and Y_P are the permanent components of consumption and income, L_T, the transitory component of liquid assets. He further assumes that

$$r_{C_P C_T} = r_{Y_P Y_T} = r_{Y_T C_T} = r_{C_T L_P} = r_{C_T L_T} = 0, \qquad (2)$$

where r is the correlation between the variables indicated by subscripts. These yield his equations (2.40) and (2.41).[1] We can make his model more powerful by adding the assumptions:

$$r_{Y_P L_T} = r_{Y_T L_T} = 0. \qquad (3)$$

The first of these subsidiary assumptions seems no less reasonable than those in (2); the second is more dubious. Presumably, the transitory component in *initial* liquid assets is correlated with the preceding year's transitory component of income—on the permanent income hypothesis, the transitory component of income does not affect consumption, hence is fully reflected in measured savings and hence in one or another item in the consumer unit's balance sheet. Now, the assumption that this year's transitory component of income is uncorrelated with this year's permanent income does not mean that it is uncorrelated with last year's transitory component of income. Whether or not it is depends on the length of the horizon implicit in the permanent component of income. If this horizon is longer than two years, as the empirical evidence suggests it is, one would expect a positive correlation between transitory components in successive years and hence, indirectly, between L_T and Y_T. However, I have made an estimate from some U.S. data of the correlation between transitory components of income in successive years and the estimated correlation is only .25. The correlation between L_T and Y_T would be smaller yet. Hence, if the U.K. relations are not very different, the second subsidiary assumption may not be too bad an approximation.[2]

If we make assumptions (3), Fisher's equations (2.40) and (2.41) greatly simplify and yield an unambiguous conclusion about the biases in b and c as estimates of β' and γ: b is less than β' and c greater than γ, provided β' is positive. Since the computed value of c is uniformly very close to zero, this strengthens the likelihood that γ does not differ significantly from zero.

If assumptions (2) and (3) are precisely satisfied, the normal equations for computing the parameters b and c of Fisher's equation (2.39) become

$$b + \lambda P_Y c = \beta P_Y, \qquad (4)$$

$$b P_L + \lambda c = \beta P_L + \gamma \lambda (1 - P_L), \qquad (5)$$

where P_L is the fraction of the variance of liquid assets contributed by the permanent component; and λ is $\frac{L_P}{Y_P}$, or the desired ratio of liquid assets to permanent income, assumed, as Fisher implicitly does, independent of permanent income. If we solve these for b and c we get Fisher's equations (2.40) and (2.41) in modified form:

---

[1] Equation (2.41) contains an error as published. On the right hand side, the first minus sign should be replaced by a plus sign, and the second γ should be replaced by β'.

[2] In some early work on his model about which Fisher wrote to me, he made assumptions (3). In writing him, I questioned them, though without giving my reasons. At the time, I did not realize that his L was initial liquid assets and (3) would certainly be unacceptable for terminal liquid assets. I owe him an apology if my offhand comments contributed to his not following the trail that he set out on and that I am now inclined to believe fruitful.

$$b = \frac{P_Y(1-P_L)(\beta-\gamma\lambda)}{1-P_YP_L}, \tag{6}$$

$$c = \frac{\beta P_L(1-P_Y)+\gamma\lambda(1-P_L)}{\lambda(1-P_YP_L)}. \tag{7}$$

The expression $\beta-\gamma\lambda$ is Fisher's β'. It follows from these equations, plus the fact that P_Y and P_L are less than unity, that $b<\beta$ and $c>\gamma$ if $\beta>\gamma\lambda$, *i.e.*, Fisher's β' is positive as it may be expected to be.

What use can we make of these equations for empirical work? For any group of units for which we can assume mean transitory components to average zero, we can approximate β by the ratio of average consumption to average income, λ by the ratio of average liquid assets to average income, P_Y by the ratio of the marginal propensity to consume, as computed from the simple regression of consumption on income, to the average propensity to consume, and, of course, b and c from the multiple regression of consumption or savings on income and liquid assets. All of these are available in the computations recorded in Fisher's paper. The still unknown items are γ and P_L.

It would at first appear that these could be estimated by solving equations (6) and (7) simultaneously. However, it is clear from equations (4) and (5), of which (6) and (7) are transformations, that this is not so. Neither γ nor P_L enters into (4); both are in the one equation (5). We shall have to proceed more circuitously.

We may note, first, that the procedures suggested give estimates of all the items in equation (4). Thus we can use this equation to test the assumptions so far made. From equation (4),

$$P_Y = \frac{b}{\beta-c\lambda}. \tag{8}$$

Suppose we compute P_Y from this equation using the methods suggested above of estimating the other parameters. We can then compare this with the value of P_Y yielded by the simple regression of consumption on income. If our model and the assumptions are valid the two should be roughly equal. I have made this calculation for those age groups and occupations that are not suspect because of a negative marginal propensity to save (Table 1). The model clearly passes this test with flying colours.

Given that this is so, we can proceed with somewhat more confidence to use equation (5). To do so requires estimating P_L or γ or both. An estimate of P_L can be constructed by a method similar to that by which we estimated P_Y. Compute the regression of income on liquid assets. The product of its slope and our estimated value of λ is then an estimate of P_L. Inserting this estimate of P_L into (5) would then yield an estimate of γ. I have not been

Table I
Estimates of P_Y and P_L

Occupation and Age Group	*Value of P_Y as estimated from*		*Value of P_L assuming $\gamma = 0$*
	Simple Regression	*Equation* (8)	
Manual			
3 (34–44)	1.00	1.01	5.4
4 (45–54)	1.00	.98	.55
Clerical and Sales			
2 (25–34)	.90	.89	.27
3 (35–44)	.89	.86	.35
4 (45–54)	.94	.93	−.20
5 (55–64)	1.00	1.00	1.4
Managers			
2 (25–34)	.82	.82	.12
3 (35–44)	.82	.82	.05
4 (45–54)	.74	.74	.03
5 (55–64)	.90	.92	−.73
Self Employed			
2 (25–34)	.36	.38	.06
3 (35–44)	.98	.98	.04
4 (45–54)	.77	.79	.10
5 (55–64)	.57	.57	.004
6 (65 and over)	.70	.71	.10

able to do this since the tables in Fisher's paper, unusually ample though they are, do not provide the requisite information.[1]

The one other thing I have done is to compute the values of P_L that would be yielded by (5) if γ could be set equal to zero. By definition, P_L should be between zero and unity. Four of the 15 values in Table I are outside this range. None of the other values seems implausible; the decidedly lower values of P_L for managers and self-employed than for manual and clerical and sales seem eminently reasonable. I conclude that this piece of evidence is also on the whole consistent with a γ of zero.

This analysis holds, of course, equally for total assets; it is simply necessary to interpret L as standing for total rather than liquid assets. However, the second of the two subsidiary assumptions in (3) is even more dubious for total than for liquid assets.

6. Pages 244–45. *The effect of liquid assets on S'.* The preceding comments are all for savings excluding purchase of durable goods (S). The results for savings including purchase of durable goods (S') are much more difficult to interpret.

Take, first, the regression of durable goods expenditures alone on income and liquid assets—this is given by the difference between the matching regressions in the upper and lower halves of Fisher's table 2.7. One would clearly expect the coefficient of the liquid asset term to be positive. Part of durable goods expenditure is consumption expenditure—the part corres-

[1] Note that the slope of the regression of liquid assets on income divided by the estimated value of λ is an independent estimate of P_Y that can be checked with the other two in Table I. A marked difference between this estimate and the others would cast especial doubt on the assumption that λ is independent of Y_P.

ponding to (*a*) the use value of the services rendered during the survey year by the new items purchased plus (*b*) the expenditures on new durables required to make good depreciation on the initial stock of durables. This part, which is likely to be an appreciable fraction of the total, should behave like other consumption expenditure and hence would be expected to rise with liquid assets for precisely the same reason that we expect negative coefficients of L in the upper half of Table 2.7. The rest of durable goods expenditure is either a net accumulation of assets, not matched by changes in other asset items, or a change in the structure of assets. Insofar as it is the latter, one would expect it to be large when initial liquid assets are relatively high and conversely, since the high level of initial liquid assets would on the average be above desired levels. For a single item in the balance sheet like durables, this substitution effect might be expected to dominate any expansion effect. The actual coefficient is in the expected direction with six exceptions (managers, three top age brackets; self-employed, 35–44, 55–64, and 65 and over age brackets).

Let us consider, next, the partial regression of S' on L. The crucial issue here is the relative importance of the two parts of durable goods expenditures just distinguished—consumption expenditure, and what, for simplicity, I shall call asset expenditure. The sum of S plus the asset expenditure alone would be expected to yield a negative coefficient—insofar as the asset expenditure represented a change in the structure of assets it will have been balanced by a decline in some other asset item that would have entered as a negative component of S; as a result only the net accumulation would show up in S'. But, as we have seen, the consumption expenditure part would yield a positive coefficient. Hence it seems impossible to say *a priori* what the sign of the coefficient of L would be for S' as the dependent variable. I intepret the actual results in Table 2.7 as meaning that, except possibly for the self-employed, consumption expenditure constitutes a large part of total durable good expenditure and hence that S is closer to the desired concept than S'.

7. Page 246. There is a reason in addition to those which Fisher cites for expecting size of family to be negatively related to saving for given measured income. Size of family, like liquid assets, is in part a proxy variable for permanent income. On the average, income, both measured and permanent, is positively related to size of family, partly because a larger family tends to mean more earners, partly because the size of family, as defined for the Savings Survey, is on the average largest during the peak earning period of the principal earner—when he is younger than this, he is likely not to have had all his children, when he is older, some or all of his children are over eighteen and so considered as separate units. In consequence, for a group of units with the same measured income, permanent income and hence, on the permanent income hypothesis, permanent consumption will tend to rise with size of family. As with liquid assets, it is difficult to separate this proxy effect of size of family on the regression coefficients in Table 2.8 from the effect of size of family in its own right.

Another factor that may be important in judging the results in Table 2.8 is the mixture of the different types of units discussed above in point 3—primary units without secondary units present, primary units with secondary units, and secondary units.

8. *Savings and the structure of assets.* Fisher's analysis of the role of liquid assets has led me to consider more fully than I previously had, though still most inadequately, the problem of co-ordinating the theory of assets and the theory of savings—my own previous work had been directed much more to explaining *consumption* than to analyzing savings and their disposition. Accordingly, I should like to supplement the above detailed comments with some more general observations.

As of any point of time, the consumer unit has some definite, and observable, balance sheet containing assets and liabilities of various kinds and amounts. In interpreting his behaviour, we also suppose him to have some expectations about the future and a structure of tastes and preferences. These are not observable directly. The role of a theory is to infer them from his observable behaviour, or, more precisely, from the observable behaviour of a group of units. The permanent income theory supposes that we can summarize his expectations by something called permanent income and can express his tastes as a function of observable characteristics of the consumer unit and his environment. Given his tastes, the consumer unit is viewed as adjusting his consumption to his permanent income, his balance sheet, and other variables, as *planning* to consume specified amounts and to add specified amounts to his assets. He is viewed as not permitting his consumption plan to be affected by differences between what statisticians measure as his income and what he regards as his permanent income. But this immediately has implications for the balance sheet. If the consumer unit does not increase consumption when his measured income exceeds his permanent income, the excess must add to one or another asset in his balance sheet or subtract from liabilities; if he does not curtail consumption when his measured income falls short of his permanent income, or even short of his planned consumption, he must finance the difference by drawing on assets or increasing liabilities. We are thus led to associate changes in balance sheet items not only with *planned* savings, taken to be a function of permanent income, but also with transitory income, and, in a full analysis, transitory consumption as well. Presumably, different balance sheet items will play decidedly different roles in absorbing transitory components, and one direction of development of the theory will need to be to formulate these linkages explicitly.

This is satisfactory for a moment in time, but it is inadequate over any substantial period, for it would make the structure of the balance sheet a will-o'-the wisp, at the mercy of the time sequence of transitory components of income and consumption. But we must surely suppose our consumer unit to plan his balance sheet as well as his consumption. Partly, this is allowed for in the present theory, by making the ratio of permanent consumption to permanent income a function of the balance sheet—as when Fisher makes permanent consumption depend on the transitory component of liquid assets—and to some extent, the extension of the theory will doubtless take the form of a more specific elaboration of this feed-back process. But it may well be desirable to consider more sophisticated feed-back processes, particularly *via* permanent income itself. Some transitory components will be regarded as temporary in the sense that they will shortly be balanced by others of opposite sign. But some may well be regarded as changing permanent income, like the windfall of an unexpected inheritance. A fully satisfactory theory would close the circuit by containing within itself an explicit theory of the formation of expectations.

All this takes the existence of the consumer unit for granted. But a consumer unit is subject to deliberate formation, dissolution, and expansion or contraction as well as to accidental formation, termination and change. In a broad sense, this too, is a question of the balance sheet, only this time of human resources. It is the part of the problem on which the least has probably been done to date and about which, therefore, it is hardest to be at all explicit. But it is also a part of the problem that will become urgent as soon as any extensive bodies of data are available covering a sizeable span of consecutive years for the same individuals.

In closing, may I say how much I welcome the use of the Savings Survey data to test, revise, and extend the recently proposed theories of savings. These theories offer the promise of a real break-through that will enable us to go well beyond the largely descriptive multiple correlations that have become so discouragingly complex in the course of time and to arrive at an understanding of savings behaviour in terms of a few fundamental forces whose manifestations can be studied empirically. They may not live up to this promise but there is already enough evidence in favour of the common central idea to justify further experimentation with them. This further experimentation is very much needed both to facilitate a choice between the different specific embodiments of the central idea that have been suggested and to enable improvements to be made in them. They are still pilot models, not yet ready for the assembly line. It is therefore much to be hoped that further tests of this kind will be made with future Savings Survey data. Such tests will be especially valuable if they are based on data giving savings and income for a number of successive years for the same consumer units. Some of the most powerful implications of the permanent income hypothesis are about the relation to be expected between income variability from year to year and consumption behaviour and can be tested only with such data.

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EXPECTATIONS AND CONSUMER BEHAVIOUR

By T. P. Hill

This comment will be concerned with certain aspects of the theory of consumer behaviour proposed by Modigliani and Brumberg[1] which do not seem to have been sufficiently elaborated either by the authors or by Fisher in his *résumé* of their hypothesis. Certain implications of the theory will be developed more fully in this comment since they have a direct bearing on the interpretation of the sort of data examined by Fisher in his article.

The Modigliani-Brumberg hypothesis places great emphasis upon an individual's expectations and planning over his entire lifespan. It is argued that the level of consumption in a given period is determined by two factors; firstly, the size of aggregate expected resources over the lifespan, and secondly, the inter-temporal consumption plan. The latter is assumed to be formulated independently of the former and to depend on personal tastes only. Despite the great importance attached to expectations and planning, uncertainty is deliberately excluded from the model. As a result, current income is relegated to a very minor role in determining current consumption, since the expected income in each and every subsequent year of an individual's life is made to carry as much weight as current income in deciding current consumption, except in so far as current income may modify expectations about future incomes.

Since consumption is assumed to be a function of expected total resources over the lifespan, changes in the level of current income which do not lead to a revision of expected total resources ought not to have any effect on the level of current consumption. Clearly, changes in current income which occur exactly as anticipated initially must come into this category, and only unexpected changes in income ought to cause a revision of aggregate expected resources. Thus, the individual's reaction to expected changes in income should be fundamentally different from his reaction to unexpected variations in income. For example, under the conditions postulated by Modigliani and Brumberg, an increase in income which was correctly anticipated ought not to induce any additional consumption over and above what was planned, because consumption in preceding periods should have been higher than it would have been if the current increase in income had not been expected. Since any actual (or measured) change in income will be a compound of both an expected and an unexpected change in income, it is necessary to allow for the extent to which the actual change was correctly anticipated in deducing how the consumer will react. No systematic attempt is made to do this by Modigliani and Brumberg, or by Fisher, in arriving at their *a priori* savings and consumption propensities; indeed, the formulae which they deduce relate only to wholly unexpected changes in income and as such are not generally applicable to actual changes in income. It is now proposed to reformulate the marginal propensity to consume in such a way as to bring into relief the role of previous as well as current expectations about income and to consider how far this affects the explanation of the cross-section rates of consumption and savings advanced by Modigliani and Brumberg and subjected to indirect testing by Fisher.

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[1] Modigliani and Brumberg, ' Utility Analysis and the Consumption Function ', *Post-Keynesian Economics*, ed. K. K. Kurihara, pp. 392–436.

It is necessary first to recapitulate very briefly the main features of the Modigliani-Brumberg theory. Their basic assumption is that consumption is a function of total expected resources over the remaining life span, the proportion of these resources which an individual plans to devote to consumption in any particular period being assumed to be determined only by his tastes and not by the size of his resources. Adopting their symbols, we may write:

$$c^t_\tau = \gamma^t_\tau \, v^t_t \tag{1}$$

where γ^t_τ depends on the specific form of the utility function (and on the rate of interest) but is independent of v^t_t, the total expected resources from time t. Throughout, the superscripts will denote the period in which the expectations and planning are taking place, while the subscripts will denote the period to which the expectations or planning refer; when both periods are the same, the superscript will be omitted for simplicity. Aggregate expected resources, v_t, are equal to accumulated assets at the beginning of period t plus aggregate expected incomes over the remainder of the earning span $N - t + 1$. If we make a similar set of simplifying assumptions to those of Modigliani and Brumberg—namely that the individual plans to consume the whole of his resources and not to leave a legacy, that interest rates are zero, and that all the γ^t_τ are the same—it follows that planned consumption in each period of the remaining life span is constant and equal to $\frac{v_t}{L_t}$ where L_t denotes the remaining life span $L - t + 1$.

If expectations about future incomes are exactly realised in practice, actual consumption will be the same in consecutive time periods so that total resources will diminish from period to period by the same proportion as the remaining life span. Expected total resources in period t as anticipated from period $t - 1$ can evidently be written as follows:

$$v^{t-1}_t \equiv v_{t-1} - c_{t-1} = v_{t-1} \frac{L_t}{L_{t-1}} \tag{2}$$

However, if the change in income between t-1 and t turns out to be different from what was anticipated in period t-1, a revision of expectations about aggregate resources ought to occur. Let us use the operator U to denote the unexpected change in a variable. Thus, by definition,

$$\mathrm{U}v_t \equiv v_t - v^{t-1}_t = v_t - (v_{t-1} - c_{t-1}) \tag{3}$$

We continue to assume that the individual plans to consume the whole of his expected resources at an even rate over his remaining life span, so that consumption in period t can be written as,

$$c_t = \frac{v_t}{L} = \frac{v_{t-1} - c_{t-1} + \mathrm{U}v_t}{L - t + 1} \tag{4}$$

On substituting c_{t-1} (L—t+2) for v_{t-1}, we have

$$\Delta c_t \equiv c_t - c_{t-1} = \frac{\mathrm{U}v_t}{L - t + 1} \tag{5}$$

As might be expected, equation (5) shows that planned consumption is adjusted so as to spread the unexpected increment (or decrement) in aggregate resources evenly over the remaining life span. When nothing occurs to upset previous expectations, Δc_t must, of course, be zero, which accords with the underlying assumption about the consumption plan.

$\mathrm{U}v_t$ must be equal to the unexpected portion of the change in current income plus the aggregate change, if any, in expected incomes over the remaining earning span $t + 1, \ldots . N$. Thus we may write:

$$\mathrm{U}v_t = \mathrm{U}y_t + \sum_{\tau=t+1}^{N} (y^t_\tau - y^{t-1}_\tau) \tag{6}$$

where $\mathrm{U}y_t \equiv y_t - y^{t-1}_t$

It is necessary to introduce two coefficients at this point. Firstly, a coefficient λ is defined which indicates the proportion of the actual or measured change in current income which was unexpected; that is,

$$\lambda \equiv \frac{y_t - y^{t-1}_t}{y_t - y_{t-1}} = \frac{\mathrm{U}y_t}{\Delta y_t} \tag{7}$$

Secondly, a coefficient η is defined which measures the elasticity of expectations about future incomes with respect to an *unexpected* change in current income; that is,

$$\eta = \frac{\sum\limits_{\tau=t+1}^{N} (y^t_\tau - y^{t-1}_\tau)}{\sum\limits_{\tau=t+1}^{N} y^{t-1}_\tau} \div \frac{\mathrm{U}y_t}{y_{t-1}}. \tag{8}$$

Equation (6) can now be rewritten as follows:

$$\mathrm{U}v_t = \lambda \Delta y_t \left\{ 1 + \eta \frac{\sum\limits_{\tau=t+1}^{N} y^{t-1}_\tau}{y_{t-1}} \right\} \tag{9}$$

Let us replace the term $\dfrac{\sum\limits_{\tau=t+1}^{N} y^{t-1}_\tau}{y_{t-1}}$ by the symbol w. It follows from equations (5) and (9) that the m.p.c. can now be written as,

$$\frac{\Delta c_t}{\Delta y_t} = \frac{\lambda}{L_t} \left(1 + \eta w \right) \tag{10}$$

The properties of w may be briefly noted; it can be seen to be a number of the same order of magnitude as the number of periods in the remaining earning span, tending to be rather greater than the latter when higher incomes are expected in the future, and *vice versa.*

Equation (10) exhibits the essential characteristics of the observed m.p.c of an individual behaving in accordance with the set of assumptions listed above. For example, when there is no unexpected change in income, $\lambda = 0$ and the m.p.c. is also zero; on the other hand, if the change in income is wholly unexpected and is considered to be purely transitory, $\lambda = 1$ and $\eta = 0$ so that the m.p.c. is equal to $\frac{1}{L_t}$. This corresponds to the equation on page 398 of the article by Modigliani and Brumberg and to equation (2.4) of Fisher's; it also corresponds to example II in Fisher's table on page 222 of his article.

From equation (10) it is evident that the extent to which the actual change in current income was correctly anticipated (which is measured by the coefficient λ) exerts a direct influence on the observed m.p.c. This point is never made explicitly by Modigliani and Brumberg, or by Fisher, who seem to concentrate exclusively on wholly unexpected changes in income. This omission assumes greater importance when we allow for the fact that λ can take any value between plus and minus infinity. Thus, λ is positive and greater than unity whenever the actual change in income is in the opposite direction to that expected; on the other hand, λ is negative whenever the actual change in income is less than was expected, provided that both changes are in the same direction. Under these not very improbable conditions, therefore, the m.p.c. itself might easily be greater than unity or negative—given the underlying assumptions of the model.

For an individual of a given age, the observed m.p.c. will depend largely on the interrelation between λ and η[1]. *A priori* reasoning suggests that the two coefficients might tend to be negatively correlated on the grounds that the sort of changes in income which are expected to be permanent are likely to have been roughly anticipated, whereas largely unexpected changes in income are likely to be considered transitory. Indeed, the implications of a positive correlation between λ and η are rather implausible. For example, if the actual increase in income turns out to be less than was anticipated (λ negative), the individual is not likely to react by revising his expectations about future incomes in an upward direction (η negative). If λ and η are in fact negatively correlated, the m.p.c. will always tend to be close to zero and sometimes negative, which is patently unrealistic, since it in turn implies that the marginal propensity to save will always be close to unity, being sometimes greater than unity. Modigliani and Brumberg seek to escape this conclusion by arguing that the m.p.c. will be quite large when the change in income is expected to be permanent. However, this explanation is not satisfactory if the above arguments are valid.

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[1] In theory, there are no limits to the range of variation of η as well as λ, but η is far less likely than λ to lie outside the range 0–1 in practice. It should also be noticed that η differs from the elasticity of income expectations defined by Modigliani and Brumberg, *op. cit.*, p. 413.

So long as the assumption that the individual plans to consume his aggregate expected resources at a constant rate is retained, the m.p.c. will almost inevitably be very small, since otherwise it is necessary not only for the actual change in income to be regarded as permanent but also for it to be largely unexpected. However, if we drop the assumption of a constant planned rate of consumption—which is only a simplifying assumption and is not an essential part of the hypothesis—much larger propensities to consume become feasible. As soon as we admit the possibility that the planned rate of consumption may vary from period to period, the crucial factor becomes the correlation between changes in expected income and changes in planned consumption. For example, if they happen to be perfectly correlated and expectations are continuously realised, the observed m.p.c. may be unity—exactly the opposite conclusion to that reached earlier.

In general, given only the basic assumptions of the Modigliani-Brumberg hypothesis, the observed m.p.c. for an individual over time will depend largely on three sets of factors:

(1) the degree of correlation between planned consumption and expected income. While in practice some correlation may exist, from a theoretical point of view it can only be regarded as spurious, since consumption plans are supposed to be formulated quite independently and to depend on the individual's tastes only,

(2) the extent to which expectations are realised in practice,

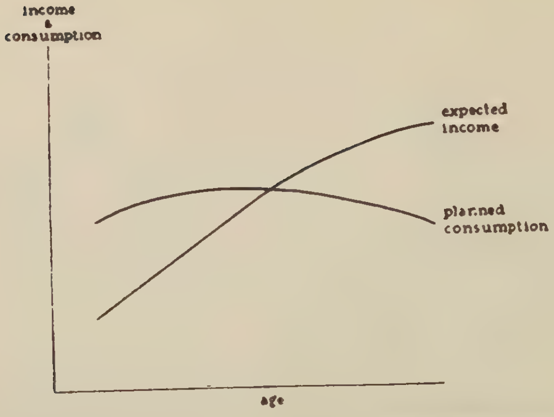
(3) the magnitude of the revision in expected aggregate resources consequent upon an unexpected change in income, which in turn depends mainly on the individual's age and elasticity of income expectations as defined above.

Ignoring the complication of secular rises in real income over the lifespan of an individual, the typical life cycle in consumption and income is for both to reach a peak around the middle of the earning span. If we follow Modigliani and Brumberg in assuming that there is no simple or direct relationship between current consumption and current income, this phenomenon might be explained simply by arguing that it is in the middle of the earning span that both family commitments and earning capacity happen to reach their maxima. There are grounds, therefore, for expecting observed marginal propensities to consume to be around unity, at least on average. In individual cases, however, practically any result is possible; for example, the m.p.c. of individual whose consumption plans and income expectations are similar to those depicted in Diagram I would tend to become progressively smaller as he grew older, finally becoming negative in the higher age groups.

Once we allow for the simultaneous and independent variation in planned consumption and expected income, it becomes difficult to generalise about whether the effect of unexpected variations in current income will tend to depress or inflate the observed m.p.c. since the m.p.c. out of the unexpected variation in income may or may not be (algebraïcally) greater than the original planned m.p.c.

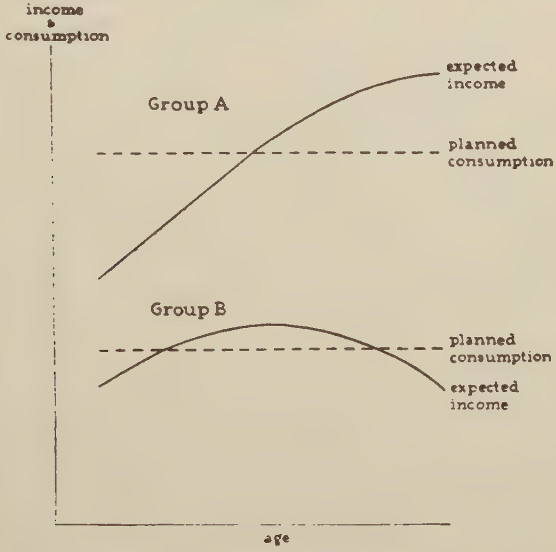
Although the above discussion has been conducted in terms of discrete

Diagram I



period analysis—unlike the original exposition by Modigliani and Brumberg—it is still possible to draw certain inferences about the observed marginal rates of consumption for a cross-section of individuals or households within a single time period. Fisher, following Modigliani and Brumberg, argues that the reason why the proportion of current income which is saved tends to increase with income in a given cross-section is that the level of current income will tend to be correlated with the amount of any unexpected increase in income received. While it is true that, *ceteris paribus*, individuals who have received an unexpected increase in income will tend to have a higher savings/income ratio than those who have experienced an unexpected decline in income, the explanation just given does not seem altogether satisfactory for two reasons. Firstly, it is inherently paradoxical to base the explanation on *unexpected* variations in income when uncertainty is specifically excluded from the model. Thus, it is necessary for actual changes in income to diverge from previous expectations on a considerable scale if the explanation is to carry much weight, and yet individuals are supposed to continue to plan for the future with complete certainty. In other words, individuals must not learn from experience. Secondly, there does not seem to be any good reason for ignoring possible differences in the savings/income ratio even when expectations are fully realised. If people with different expected aggregate resources—or, alternatively, with different *average* expected incomes—tend to have different expected life cycles in income, the savings ratio will tend to vary with income in a cross-section, even though the observations are confined to a single age group. This possibility has to be admitted even though consumption plans are, *ex hypothesi*, assumed to be formulated independently of the size of aggregate expected resources. In fact, there is evidence to show that the typical life cycle in income differs considerably between different occupational groups and, in particular, between groups with different levels of average expected income. Thus, the proportionate variation in the remuneration of ' managers ' over the earning span is greater than that for manual workers; ' managerial ' incomes rise more steeply at young ages and do not decline so markedly in the later age groups. Moreover, even within the manual category, the life cycle for skilled workers differs somewhat from that for unskilled workers. The implications of different life cycles in income are illustrated in Diagram II, where for convenience we assume a constant rate of consumption for both sets of individuals.

Diagram II.



Firstly, if expectations are broadly realised, it is obvious that, for each group separately, the savings ratio will vary with age and, *a posteriori*, with income whenever we do not control age. Secondly, however, and this is the more interesting possibility, the savings/income ratio will vary with income at a given age if we pool both groups together. Thus, for the two groups in Diagram II we would expect to observe a *negative* cross-section marginal rate of savings at young ages and a positive rate at older ages. Thus, it is quite feasible for the savings ratio to vary with income even though no unexpected changes in income occur whatsoever.

In general, therefore, the observed cross-section marginal rates of consumption will depend largely on four factors.

(1) Whether or not age is controlled.

(2) Differences in expected life cycles in income for individuals with different average expected incomes over the earning span.

(3) The extent to which these expectations are realised in practice.

(4) The strength of the correlation between the level of current income and the amount of any unexpected change in income received.

Conclusion

In the first part of this comment it has been argued that insufficient attention is paid by Modigliani and Brumberg, and also by Fisher, to the distinction between expected and unexpected variations in current income. Even though the individual is supposed to formulate his consumption plans and income expectations in each period under conditions of subjective certainty, it is still necessary to allow for the fact that previous expectations about the level of income in the current period may not be exactly realised in practice. Since consumption plans are assumed to be drawn up independently of the expected income stream, it is impossible to establish any functional relationship relating changes in current consumption to changes in current income in so far as the latter conform to previous expectations. It is only when the change in current income is such as to cause a revision of expectations about aggregate resources that the change in current consumption can be directly affected by the change in income. The distinction between expected and unexpected changes in income is, therefore, as important as, and is complementary to, that between permanent and transitory changes in incomes. If we compare two individuals with the same utility function and the same *ex post* life cycle in income, their actual consumption behaviour will still differ if one is a better prognosticator than the other. Even though subjective uncertainty is not allowed to influence the consumer's planning, objective uncertainty will still creep into the model.

In the second part of this comment, it was argued that differences in expected life cycles in income between different individuals or groups must play a large part in explaining observed cross-section rates of consumption or savings. It might be possible, for example, to produce an acceptable explanation for certain ' freak ' results, such as negative cross-section rates of savings, along these lines. Differing degrees of objective uncertainty attaching to the expected life cycles in income for different categories may also contribute to explaining the observed cross-section rates of savings. This possibility is closely analogous to the Friedman hypothesis.

It should be emphasised that no attempt has been made in this comment to depart from the fundamental assumptions of the Modigliani and Brumberg hypothesis. However, it is extremely debatable how realistic they are in practice and the model might be greatly improved, although greatly complicated, by relaxing them. In reality, the motivation behind consumer behaviour is likely to be fairly complex, so that the quest for simplicity does not necessarily take one nearer the truth. The question of whether or not subjective uncertainty (or the level of current income) exerts a direct influence on current consumption is a simple question of fact not to be decided by a misguided appeal to the principle of Occam's razor. Lifespan planning and expectations no doubt play a very important part in influencing consumer behaviour, but this is not sufficient justification for trying to remove the causal nexus between current income and current savings altogether.

ECONOMETRIC ANALYSIS OF THE SAVINGS SURVEY DATA

By TRYGVE HAAVELMO

I

How much empirical knowledge do we actually have concerning the propensity to save or concerning savings patterns in general? Judging from the enormous volume of macro-economic literature using the propensity to save (in some form or other) as a basis, one would think that economists had already established solid empirical relations in this field. However, the actual situation is very far from such a happy status. It is true that high correlations have sometimes been found between time series of savings and of income. But the *degree of permanence over time* of such relations has generally not been impressive. Econometricians have been hoping to get better empirical information on savings patterns from sample surveys, in particular when such surveys could be conducted on a large scale. The results of the comprehensive Savings Surveys in the United Kingdom that are now available, will certainly be studied eagerly by econometricians the world over.

There are at least three important reasons why cross-section data on savings are essential as alternatives to, or as supplements to, time series data. One reason is that the average disposable income of consumers seldom shows sufficiently large short-run variations to permit a reasonably accurate measurement of the propensity to consume. A second reason is that cross-section data can tell us a good deal about how to develop approximate aggregation formulae. A third reason is that cross-section data can give us information on the effects upon savings of certain variables (age, family size, etc.) that do not vary at all, or vary only slightly, in aggregate time series for the community as a whole.

It is of course true that the use of survey data requires some rather bold hypotheses concerning individual ' likeness ' under ' similar conditions '. But that is also to some extent true for a study based on time series, in the sense that then one usually has to build on some hypothesis of inter-temporal (instead of inter-personal) comparability.

The analysis presented in Dr. Fisher's article is based on a number of regression studies. Certain savings theories developed by Modigliani and Friedman have been tested against the available data by means of methods that require statistically specified models. Tests of significance have been applied in order to study the importance of variables such as age, occupation, liquid assets, etc. It is clear that such procedures must have as their foundation some stochastic specification of the ' universe ' from which the survey data are drawn. It seems, however, that the practice in sample survey projects is to omit such precise stochastic specifications. That is also true in the present case. The idea that an explicit specification of the stochastic model is necessary may not be shared by all econometricians. For my own part I have found it almost unavoidable to go into this matter in some detail, in order to interpret some of the results presented in Dr. Fisher's article. I shall try to outline very briefly the kind of specification that seems natural in the present case.

II

Consider the rate of savings of a particular individual, say No. j, at a certain point of time t. On the basis of traditional principles in economic theory we are led to the hypothesis that the rate of savings is ' determined ', although with some degree of uncertainty, by a process of rational choice which depends on the preferences of the individual, and on certain ' outside facts ' (such as income, wealth, etc.) which are data to the individual.

The relation between the rate of savings and the ' outside facts ' may contain very large unobservable random disturbances even if a great many ' outside facts ' are taken into account explicitly. And, what is perhaps even worse, the relation may differ in form from one individual to the next, *e.g.* because of differences in the preference structures. What we are looking for when we try to disclose ' patterns of savings ' is, of course, a relation which ascribes as much as possible of individual differences in the rate of savings to differences in observable ' outside facts '. To achieve this, we may try to make not only the ' conditions of choice ' but also the individual preference functions compatible by introducing variable parameters, such as age, family size, etc. to account explicitly for individual differences. Let us consider what kind of explicit variables it may be necessary to introduce in order to arrive at such a general savings function, *i.e.* a savings function the form of which does not depend on the number j (and, preferably, not on the point of time t either).

I think we are led to consider the following types of explicit variables:

(*a*) Variables that may *differ from one individual to the next*. Let $y_{1j}^{(t)}$, $y_{2j}^{(t)}$, ..., $y_{kj}^{(t)}$ denote observable variables of this kind, such as income, liquid assets, age, etc., and let $u_j^{(t)}$ denote a ' catch-all ' for those that are not observable. We shall assume here that the $u_j^{(t)}$ are random variables of some kind.

(*b*) Variables that are *the same for all individuals* and depend only on t. Let $z_1^{(t)}$, $z_2^{(t)}$ ..., $z_m^{(t)}$ denote observable variables of this kind (such as prices, taxes, etc.) and perhaps time itself, and let $v^{(t)}$ denote a 'catch-all' for those that are not observable. We shall assume here that $v^{(t)}$ is a random variable of some kind.

(*c*) Random variables $w_j^{(t)}$, one for each individual, which represent the possibility of some individual random variation in the choice of the rate of savings even when all the variables under (*a*) and (*b*) are fixed. One might question whether such variables are necessary in addition to the variables $u_j^{(t)}$. The difference is that the u's represent characteristics which are given to the individual and may follow him more or less throughout his lifetime, while the w's are essentially changeable by the individual himself, as time goes on. In other words, the *u's* may have a high degree of serial correlation (perhaps perfect correlation), while the *w's* may have little or no such correlation.

Our hypothesis concerning the rate of savings $s_j^{(t)}$ could then be stated in the following form

$$s_j^{(t)} = F(y_{1j}^{(t)}, \ldots, y_{kj}^{(t)}, u_j^{(t)}, z_1^{(t)}, \ldots, z_m^{(t)}, v^{(t)}, w_j^{(t)}), \qquad (1)$$

where the form F is independent of j and t.

The task of an economic theory of savings, then, is to specify the form and the variables of the function F, and also the stochastic properties of the variables u, v and w for given values of the y's and the z's. The task of statistical research is to test this specification against a sample of data for savings and the corresponding data for the y's and z's. Or, if the form F is assumed known, the problem may be one of estimating unknown coefficients.

The data we get by means of a sample survey may be considered as results of the following process: First, the working of the economy as a whole determines for the point of time t a certain ' population of savers ' for the nation, that is, a set of given values for the variables y, z, u, v, w for each individual saver in the whole economy. This is a very complicated process, partly random, partly perhaps deterministic.

A set of sample survey data will have all the stochastic elements of the working of the economy as a whole, and in addition the new properties of randomness introduced by the principles of the sampling technique used. It is important to distinguish between those uncertainties in the econometric results that are inherent in the economic mechanism itself and those additional uncertainties that are brought about by limitation of the sample size. Let us assume that we know the ' correct ' function F and the nature of the stochastic elements involved, so that the only things that are not known *a priori* about the function F and its random elements are the values of a certain limited number of constant coefficients. Then, what can be achieved by increasing the sample size is, perhaps, to get closer and closer estimates of the unknown coefficients of the function F and of the probability distributions involved. This means *inter alia* that by using a large sample it may be possible to estimate very accurately certain *averages* of savings for the population as a whole or for certain sub-groups. But the accuracy of predicting the individual savings amounts s_j by means of patterns estimated from one cross-section sample is *limited by the uncertainty regarding the individual values of the unobservable variables u and w.* In addition, there are the random variables $v^{(t)}$. For a given value of t, $v^{(t)}$ is an unknown constant in the function F. But over time $v^{(t)}$ may show a good deal of random variation which introduces an additional element of uncertainty if we want to use the results of prediction for a point of time different from that of the survey. On the other hand, the variables u which may be very different as between individuals, *may for a particular individual remain almost unchanged for a long period of time*, a fact which makes the possibility of predicting individual saving rates better than it appears from the large spread in the cross-section data for the point of time t.

III

The model above can, I think, be used to interpret some of the facts that the analysis of the British Savings Survey data has brought forth.

One of the most striking results of the study is the *persistence of very small multiple correlations* in nearly every one of the many 'regression equations' that have been fitted to the data. Changing the specification of the 'independent variables' (the y's in the model above), adding new variables, and so forth, has a remarkably small effect on the multiple correlations. Now, this result has essentially nothing to do with the limitation of the sample size. It is of course possible that, because of the sample being limited, the correlations can 'accidentally' be below the 'true' correlations. But the correlations may also, for the same reason, 'accidentally' be above the 'true' correlations.

There is no reason why increasing the sample size should systematically improve the multiple correlations. The reason for the low correlations is simply large variances of the random variables u and w in the population as a whole. These variances could perhaps be reduced by introducing more specified variables y. However, it seems to me that the present study illustrates clearly how little each such new specified variable adds to the value of the multiple correlation coefficients considered. If the results obtained are general in this respect, the conclusion is simply that on the basis of a cross-section study we can perhaps predict with some accuracy the *average* savings of a large group of individuals having the same specified values of the variables y, but we can say very little about the savings of the individual. That is the meaning of the low multiple correlations obtained. Personally, I find this result neither surprising nor disturbing. One should also bear in mind the possibility of high serial correlation in the variables u, which means that prediction of 'next year's savings' of a particular individual may not be quite as hopeless as the low multiple correlations would seem to indicate. This could perhaps be taken into account *e.g.* by using the individual's present rate of savings as an additional variable.

Another typical result is the low degree of significance found for some of the variables y, such as age and family size, in spite of good *a priori* reasons for including them. Also, in this case I doubt very much that the sampling technique and the size of the sample are the things to be blamed. I rather suspect that the problem here goes much deeper, that is, to the very foundations upon which a theory of the form (1) is built. The question is whether it is actually possible to eliminate the dependence upon j of the function F by introducing explicit variables y in any simple manner. For some of the variables relating to savings behaviour this kind of simplification is rather intricate. Take for example the variables 'income' or 'wealth'. One might ask: Does an individual have high savings because he has a high income or does he have a high income partly because he has a preference function which means that he is by nature a thrifty person? Even more doubtful would be an assumption that wealthy and poor people have the same preferences as far as thriftiness is concerned. The variable ' age ', for example, can represent at least two very different factors, one indicating the remaining length of life, the other indicating the kind of ' history ' the individual has lived through. The latter may have had some influence on his preference structure.

These considerations should not, however, be taken as a particular criticism of the analysis as presented by Fisher. In fact, if it should not be possible to rid the function F of the subscript j in the way indicated, the outlook for general economic theory would be rather dark as far as the possibility of establishing behaviour patterns for savings is concerned.

The extensive use of least squares regressions treating the variables y as ' independent variables ' deserves a comment. Strictly speaking, such variables y as ' income ' are not independent of the random variation in individual savings because of the repercussions through the income-creating mechanism of the economy. However, when the ranges of variation of the y's are wide, it is possible that the regression bias due to these repercussions may be negligible.

IV

How permanent can we expect savings patterns to remain as time goes on? Of course, the patterns found by a cross-section analysis for one point of time (or one short period) are in any case not sufficiently general for the reason that such a study does not make it possible to measure the role of the variables z in (1), that is, the effects of price changes, changes in taxation, etc. But even if these effects could be measured by means of additional time series data, there is the fundamental problem of how permanent the savings patterns actually are. In all kinds of demand analysis there is the problem of changing tastes. Savings patterns are probably particularly susceptible to changes of this kind because of the close relation between general social and economic policy of a community and the need for individuals to save. Nearly all theories of personal savings emphasize the importance of uncertainty as affecting the propensity to save. It is clear that the rapid development in many countries towards an economy where nearly all the ' old ' kinds of individual uncertainties are greatly reduced must have a profound effect on individual attitudes towards private savings plans. Thus it may be even more problematic to assume that the functional form F in (1) can be made independent of t than it is to assume that the function is the same for all individuals. We shall at any rate probably need rather intricate variables z in the function F in order to cope with this problem.

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THE SIGNIFICANCE OF INCOME VARIABILITY ON SAVINGS BEHAVIOUR

By L. R. KLEIN *and* N. LIVIATAN

Dr. Fisher has produced many interesting regression equations by age and occupation classes, and surely these are natural groupings of survey data among the most basic classifications, but does this mass of numerical work really throw much light on the highly specific hypotheses suggested by Modigliani-Brumberg and Friedman? All the material presented by Fisher is only indirectly related to the crucial points at issue, and one feels very sceptical indeed about the use of such indirect evidence in making delicate judgments.

Fisher believes that his clearest findings substantiate the income-variability hypothesis, *i.e.*, that the departure of the Engel curve of savings from a proportional relationship can be accounted for by income variability, wealth and other variables, *but not by the level of income*. We would not like to challenge the reasonable view that income variability may have something to do with savings. Evidence of the effect of income variability has long ago been found to be relevant, in association with other factors, in determining the amount of saving.[1] We object to the particular interpretation of the data examined by Fisher and the imputation of a key or overwhelming role to income variability.

First, on the level of indirect methods of testing, we raise two criticisms of Fisher's contribution.

(1) It is not altogether clear that income variability is associated with age and occupation classes as Fisher, Modigliani or Friedman believe.

(2) In making indirect tests, Fisher gives inadequate attention to alternative explanations of the observed phenomena—explanations that are quite unrelated to the degree of income variability.

Income variability by occupation Fisher accepts as self-evident the fact the self-employed income units have more variable incomes than do other groups and as plausible, but needing proof, that managerial and technical employees are high on the scale of variability. These are the occupational groups with comparatively high savings propensities. If we examine bits of direct evidence on past and expected income change, it is by no means certain that his assumptions are correct.

In the *Surveys of Consumer Finances* (U.S. data) comparisons between the income variability of owners of unincorporated business and all other consumer units have been made.[2] It has been found in the records from four annual surveys that income expectations are more variable among businessmen than the rest of the population, but this is not true of past income change.

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[1] See G. Katona, ' Effect of Income Changes on the Rate of Saving ', *The Review of Economics and Statistics*, Vol. XXXI, 1949, pp. 95–103.

[2] See L. R. Klein, and J. Margolis, ' Statistical Studies of Unincorporated Business ', *The Review of Economics and Statistics*, Vol. XXXVII, 1954, pp. 37–38.

In some fresh tabulations from the 1950 Consumer Purchases Study of the Bureau of Labor Statistics made available to us from the Wharton School of Finance and Commerce, it is shown that self-employed units report no change of income (1949–50) and no expected change in income (1950–51) at a high rate compared with other occupational groups. Unskilled wage earners and retired and unoccupied persons have unchanged incomes most frequently, but it would be wrong to conclude from the data in Table I that the self-employed, salaried professional, and managerial units have changing incomes most frequently.

TABLE I

Percentage of Consumer Units Reporting Unchanged Past and Expected Income by Occupation Groups.
Consumer Purchases Study, 1950.

Occupation	*Past income unchanged*	*Expected income unchanged*
Self-employed	41.3	44.1
Salaried professional, managerial	35.9	42.5
Clerical and sales	35.7	42.0
Skilled wage earners	35.0	41.5
Semi-skilled wage earners	37.5	41.3
Unskilled wage earners	45.3	46.3
Retired and unoccupied	57.4	59.7
All occupations	40.8	45.1

Additional evidence on the degree of variability by occupational class is provided from the Oxford Savings Surveys. In Table II we have correlations within occupational groups between incomes reported by the same unit at the time of the 1953 Survey and the 1952 Survey. The sample underlying this table is a special reinterview sample, *i.e.* a sub-sample of 1952 respondents interviewed a second time in 1953.

TABLE II

Correlation between Net Income Reported in 1952 and 1953
Reinterview Sample, 1953

Occupation Group	*Number of cases*	*Weighted percentage of cases*	*Correlation*	*Regression coefficient*[1]
Manual, clerical and sales ...	193	71.8	.80	.83
Retired and unoccupied	54	16.3	.80	.70
Managers and technical ...	69	5.9	.94	.99
Self-employed	47	5.9	.83	.63

[1] Regression of 1953 income on 1952 income.

By the criterion of the degree of correlation between successive years' incomes, we arrive at opposite conclusions from those commonly accepted about income variability among occupations. This criterion seems reasonable if we interpret the amount by which the square of the correlation coefficient falls short of unity as a measure of 'unexplained' dispersion or random variability. The slope (and intercept) of the regression line may, however, be relevant in showing the degree to which one year's income departs from the next. If such departures are regular and expected they do not necessarily indicate variability in the sense intended for the savings theories in question. Nevertheless, a direct inspection of the regression coefficients in Table II gives no basis for placing managerial units high on the scale of variability. It does not alter our *a priori* conception about the year-to-year variability of entrepreneurial earnings. They do depart significantly from the identity relationship (income 1953 = income 1952) in this sample.

Alternative explanations of observed phenomena. In Fisher's regressions by age and occupation groupings, two results stand out clearly, the high savings propensity of the self-employed and the low savings propensity of the aged. Apart from the doubt expressed above concerning income variability among self-employed units, we may question whether the extreme savings behaviour of the two groups being considered is attributable to income variability or, as Friedman puts it, to the discrepancy between measured and permanent income of the aged. Self-employed units are high savers to a large extent because they intend to reinvest earnings in their enterprises. Difficulties of raising credit for small enterprises in imperfect capital markets and a desire to retain full control over business operations are plausible reasons for high business saving. To focus attention primarily on the variability of income to the neglect of these obvious motives is hardly defensible.

The self-employed group, in Fisher's table, has a large average propensity to save. In the face of economic uncertainty, as exemplified by their high income variability, this group would, according to Fisher, desire to hold comparatively more liquid assets in relation to income. His equation (2.36) connecting the savings-income ratio to the liquid asset-income ratio is expressed in terms of the ' excess ' liquid asset-income ratio. There is no necessary relation between a group's average desired ratio and its ' excess ' ratio of liquid assets to income. In general, we may expect the ' excess ' ratio to average to zero over an entire occupational group.

A primary reason for low saving among the aged is the frustration of the bequest motive through high death duties. In a recent sample of upper income groups, older respondents frequently cited death duties as a major deterrent to savings.[1] Several elderly persons indicated that they were willing to dissave under present tax laws in order to maintain past living standards. Dissaving is necessitated by the inflationary erosion of income, and few people look upon this as a temporary decline in real income.

A differential in savings propensities not mentioned by Fisher but cited by Friedman and which is germane to this discussion is the U.S.—U.K. differential. There is much evidence on the basis of both time series and sample survey data to suggest that the American propensity to save exceeds the British. Moreover, distributions of percentage change in consumer incomes show unmistakably greater variability in the U.S. than in the U.K.

[1] L. R. Klein, K. H. Straw, and Peter Vandome, ' Savings and Finances of the Upper Income Groups ', BULLETIN, November, 1956, pp. 293–319.

Would it be justified to conclude from these observations, as Friedman does, that support is given to his theory? Again, there are important alternative explanations. The institutional differences between the two countries in tax laws, national insurance schemes, health services, etc., are so obvious and undoubtedly have so large an impact on personal savings that it is small wonder that private savings appear to be at a lower rate in the U.K. Fisher, following in Friedman's footsteps, takes a narrow look at manifest data.

The savings/income ratio in the theories under review is allowed to depend on several factors but never the level of income. Yet differences in the levels of absolute income seem to be so much more obvious as reasons than differences in variability in explaining occupational differences. In particular, managerial units have high absolute income levels, on the average, and this would seem to be a perfectly natural explanation for their propensity to save being higher than that of other employees especially since it is doubtful that managerial incomes are more variable. In a similar way, part of the difference between savings rates in the U.S. and U.K. would seem to be explainable in terms of the American income superiority.[1]

If savings/income correlations from the 1953 Survey within occupational classes are further classified by two basic income groupings, some interesting results appear. We choose *net* income levels of below £700 and £700 or larger for the two groupings. In the lower income group, the only significant correlation is that for the self-employed group. All other respondents show insignificant negative correlations. In fact, there is much to be said in favour of accepting the hypothesis that the slope is nil in this range.[2] The self-employed are a very special group and nearly always show high correlation with a relatively steep regression gradient. In Table III we see the correlation and marginal coefficients in the group with net incomes of £700 or more.

TABLE III

Savings-income Correlations and Regression Coefficients by Occupation Classes for Respondents with Net Incomes of £700 or More

Savings Survey, 1953

Occupation	*Correlation*	*Regression Coefficient*
Manual	.08	.14
Clerical and sales	.23	.25
Retired and unoccupied	.10	.04
Managers and technical	.24	.17
Self-employed	.60	.46

The principal conclusion that we draw from this tabulation is that Fisher's findings of a high marginal savings propensity for the managerial class is due to their superior income status. At low income levels (under £700) they exhibit the same insensitivity of savings to income as do other groups who are not self-employed. At higher income levels, they are much the same as other employees in their savings habits. Only the retired and self-employed have really different patterns, and there are many plausible explanations for this.

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[1] Neither of the differences in income level cited is temporary.

[2] See p. 159 below.

An implication of Friedman's theory, not taken up by Fisher, is that savings are residual components in a consumer's budget. There is little direct evidence on this matter, but it seems very doubtful that we are justified in considering savings any more than various luxuries as a consumer's ultimate residual which bears the brunt of financial adjustment to changing situations. In any event, all of savings are not residual items. The elements of contractual savings are regarded as being as essential in the monthly budget as are items of food, clothing and other necessary consumption. The failure to go into a careful statistical distinction between contractual and discretionary savings has covered up many interesting points for Fisher and his predecessors. Among employees, a principal reason why managers and professional people have high saving-income ratios, apart from the superiority of their income position, is that they have a high ratio of contractual savings to income. Their contractual savings, in turn, are high because they frequently have superannuation schemes in connection with their employment.[1] Thus there are sound institutional reasons unrelated to income variability why they save at a high rate.

Some direct tests It is unfortunate that Fisher relied almost entirely on the indirect evidence of correlations within age and occupational groupings, interesting though they are for other purposes, instead of making more direct tests. The same bodies of data studied by Fisher could have been used more satisfactorily, in our opinion, for testing the theories in question more directly.

As one averages out cyclical fluctuations, longer run estimates of savings and income ought to tend towards proportionality. Indeed, empirical inspirations for the work of Modigliani and Friedman have been the near constancy of the savings income ratio in Kuznets' time series of decade averages for the U.S. since the latter part of the nineteenth century.[2] The long run stability of the American savings/income ratio, except for cyclical fluctuations, is similarly found by Goldsmith in his more recent studies.[3]

With sample survey data referring to one year, it is not possible to test the proportionality hypothesis by averaging out cyclical disturbances. Fisher's rays from the origin to points of mean savings and income or his computations of saving income ratios are mere *descriptions* of the theory not *tests* of it. With reinterview samples we are able to average savings and income over two years, but this still leaves us a long way from cyclical averages. Nevertheless, Friedman would like to argue that as the length of the accounting period is stretched, the saving/income relation should approach proportionality. He so argued in connection with findings from the American reinterview samples, where it was estimated that the 24 months' savings/income ratio was less strongly correlated with the level of 24 months' income than were the same variables for a 12 month accounting period.[1] The 1954 reinterview sample from the Oxford Savings Surveys does not lend support to the previous finding.

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[1] It has been found that those making superannuation contributions save more than do others in the same income classes. See L. R. Klein, 'Patterns of Savings: The Surveys of 1953 and 1954,' BULLETIN, May, 1955, pp. 173–214.

[2] S. Kuznets, *National Income: A Summary of Findings* (New York: Nat. Bur. of Econ. Res.) 1946, p. 53.

[3] R. W. Goldsmith, *A Study of Savings in the United States*, Vol. I (Princeton: Princeton Univ. Press) 1955.

Correlation between Gross Income and Ratio of Savings to Gross Income
Reinterview Survey, 1954
24 months' correlation 0.55.
12 months' correlation 0.50.

These correlations were computed by averaging savings and income within each of 28 classes of gross income. The ratio of average savings to average income was correlated with the income level. By using class means, we introduce an element of approximation, but by using as many as 28 classes we have fairly reliable guides to the relative sizes of the two desired quantities.

A second piece of more direct evidence comes from a consideration of survey data on windfalls.[2] Friend has already mentioned in his comments some relevant facts in this connection. We can possibly reinforce his findings. Friedman is elusive about his definition of permanent and temporary income, but, whatever his precise meaning can be, it seems reasonable to assume that windfalls are good examples of temporary income. He believes that they are uncorrelated with temporary consumption. Fisher tries to anticipate objections to his work by noting that positive correlation between windfalls and durable consumption does not discredit Friedman's key assumption because of the fact that durable goods are not wholly consumed in one year. Nevertheless, they are partially consumed during one year, and some real correlation between temporary consumption and windfalls remains. Having read through numerous questionnaires, especially those of a sample of recent heirs,[3] we conclude that many people spend sudden unexpected gains on travel, entertainment, and other luxuries in addition to durable goods. The case of retired and unoccupied persons is particularly relevant since they are not very much in the market for consumer durables. From the 1953 Savings Survey, we estimate the following multivariate regression for retired and unoccupied units:

$$S_d/Y = \underset{(0.143)}{0.026} - \underset{(0.061)}{0.049} \log Y/N - \underset{(0.009)}{0.030} (L/Y)(Y_{-1}/Y) + \underset{(0.0010)}{0.0011} A$$

$$- \underset{(.002)}{0.081} W/Y - \underset{(0.28)}{0.12} S_c/Y \qquad R = 0.24$$

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[1] See *Contributions of Survey Methods to Economics*, ed. by L. R. Klein, Ch. V (New York: Columbia Univ. Press) 1954.

[2] Windfalls, in the Oxford Surveys, consist of life assurance benefits, gambling winnings, cash gifts, cash legacies, post war credits and other lump sum transfers of money.

[3] See L. R. Kelin, K. H. Straw, and Peter Vandome, *op. cit.*

S_d = noncontractual or discretionary savings
Y = net income
Y_{-1} = net income of preceding year[1]
N = number of persons in the savings unit
L = beginning-of-year liquid assets
A = age of savings unit head
W = windfalls
S_c = contractual savings.

Estimated sampling errors are written in parentheses below corresponding coefficients.

It can be seen here that windfalls together with liquid assets are the most significant factors affecting discretionary savings (negatively) of these retired and unoccupied persons, most of them in advanced age groups. They are clearly using funds received in addition to income in order to enjoy a higher temporary level of living than would otherwise be possible and are definitely opposed to saving such receipts because of the influence of permanent factors like inflation and death duties.

If one is primarily interested in testing the theories under review instead of searching for general laws of savings behaviour, there are other possible ways in which the Survey data could be organised to reach the desired objective more effectively. In a freshly designed survey, people can be questioned about a longer span of their earnings and saving histories, and with a much greater input of research effort can be repeatedly interviewed over a period of a few years. Even with the present questionnaires, many forms of income and saving can be segregated on the basis of their degrees of permanence. Classification by past and expected income change within age groups would also give a longer time perspective to current estimates of income and savings. This may lead to small cells of observations in the present surveys but holds out more promise in the material mentioned by Friend from the Consumer Purchases Study in the U.S. J. Morgan's results with a sample of the same order of magnitude as the Oxford Surveys is, however, suggestive.[2] He computes savings-income regressions within a number of asset and housing status classifications for ' normal ' and other consumer units. An ' abnormal ' unit is defined as one with an income decrease in excess of 4 per cent, with an increase in excess of 24 per cent, with no job, in an age class over 65 years, or with large money transfers. On the whole, ' normal ' units have less variable income than do ' abnormal ' units. In support of the Modigliani-Brumberg-Friedman theories, the normal units have smaller marginal savings coefficients than do the others.

Appraisal Our summary view of Fisher's ' exploration ' is that he has not found empirical justification for the theories being tested, especially in their most extreme forms, but that he has provided indirect evidence for concluding, as others have previously done, that income variability is one of several factors with which we have to reckon in establishing a sound theory of saving behaviour. The stress on income variability provided in the recent contribution of Modigliani-Brumberg, Friedman, and Fisher will have the salutary effect of reminding us not to overlook this element in future work on savings.

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[1] In the ratio (Y_{-1}/Y), but not otherwise, gross incomes are used.
[2] *Contributions of Survey Methods to Economics*, Ch. III and IV.

Two technical points not mentioned previously lead us to question some of Fisher's results. His main and most firmly established finding is the comparatively high savings rate for self-employed consumer units. This is not a new fact, but it does help to have previous findings confirmed in the Oxford Surveys. We have already advanced other explanations for the high savings by entrepreneurs. but we must point out that this group presents more difficulties to the survey statistician than does any other broad occupational group. There are many reasons for being dissatisfied with the reliability of estimates of savings and income for the self-employed, and we would prefer to see Fisher's results secured on a firmer foundation.

The other technical matter which we do not find satisfactory in Fisher's study is his treatment of heteroscedasticity. He argues that failure to correct for heteroscedasticity is a matter of efficiency, which is unimportant for a sample with as many cases as the one he treats. There are strong reasons for not approving this summary dismissal of a basic problem. In the first place, the degree of linearity of the savings function is likely to be affected by the treatment of this problem. If the data are transformed to eliminate heteroscedasticity, the paths of the savings-income curves are much more regularly traced out, and one may be misled by the 'shotgun' type of scatter found in the original savings-income data. Secondly, the efficiency problem is not so easily disposed of even though the total survey sample may contain more than 2,000 individual cases. Has not Fisher overlooked the possibility that asymptotic efficiency can vary widely among different estimators? When comparing estimates obtained by a single method from different samples of varying sizes, it is correct to assume that after a given size has been reached, different estimates do not vary appreciably in efficiency. This proposition does not carry over to a given sample, no matter how large, in which parameters are estimated by different methods. As we have learned from the researches of Morgan, transformation from savings to the savings/income ratio is not as good in removing heteroscedasticity as the transformation to the ratio of savings to a linear function of income and assets. Yet the savings/income ratio is a good approximation to the desired result and would have been far more satisfactory for Fisher's regression than the forms actually used.

Modigliani-Brumberg, Friedman, and Fisher have given their interpretation of the customary shape found in survey data for the non-linear Engel curve of savings, a curve which implies that the saving/income ratio varies with income level. They think that this curve straightens out when the added dimension of income variability is brought into the picture. We can offer an alternative interpretation, one that is gained by studying many such curves from repeated surveys in Britain and America, by examining the individual questionnaire reports from which these curves are derived, and especially by looking more intensively at the returns for the upper income classes which produce all the apparent nonlinearity.

In Great Britain, from the lowest income level to about £800 per year the curve hugs the zero axis with a very gentle slope. In the lowest group there are permanently poor people who consume all their income or slightly more by drawing on past savings and borrowing. There are transients in this class who are temporarily having a bad year and consuming far more than the amounts of their low income. These are the only people who behave purely according to the theories under review. Finally, there are a number of aged persons living on capital. This is not a transient group. Next we move through the bulk of the income distribution say from £200–£800. People in this range cannot save much. They spend practically all their income and are permanently in this range. There is a small savings gradient connected with life assurance (a great deal of it industrial assurance), superannuation, some house purchase and 'small savings'. Finally, above £800, people begin to save and consider more than a limited number of savings instruments. The evidence we have uncovered from tabulations of the upper groups in nationwide cross sections and in an experimental survey of those with incomes of £1,000 or more is that the Engel curve in this region rises linearly but is not a ray from the origin. The piecing together of the gentle (almost flat) linear segment up to the level of £800 and a steeper linear segment above this point resembles a smooth curve. Most of the people above £800 are permanently in this group of active savers. They come largely from a family background in which saving is traditional. There are some temporary residents in this group, but they can hardly be described as people saving in the fat survey year to cover leaner periods. They are simply customary residents of a part of the income distribution where saving becomes possible and move along a smooth linear path of saving with the income fluctuations that do occur.

Friedman puts forward the hypothesis that the 'break-even' point (the income level corresponding to zero savings) occurs between zero and the mean income, and Fisher finds this result by fitting a single linear function to the savings-income data. Yet we feel that the nonlinearity is basic and much better represented by two linear segments joined together at a gross income level of about £800. A single straight line Engel curve fitted to the whole range of data erroneously implies strong dissaving by employees at income levels below the break-even point. According to our interpretation there is no unique break-even point for employees and one does not have to resort to Friedman's theory for an explanation.

Some equations estimated from the 1954 Savings Survey illustrate sharply the way in which the two linear functions can be pieced together to give an appearance of the typical nonlinear Engel curve. Using the same notation as before, but not using all the variables, we have computed:

Employees with incomes less than £1,000

$$S_d/Y = -2.96\,(1/Y) + 0.0066 - 0.060\,L/Y - 0.35\,W/Y$$
$$(2.79)\qquad\qquad(0.0093)\quad(0.008)\qquad\quad(0.04)$$
$$R = 0.28$$

Employees with incomes of £1,000 or more

$$S_d/Y = -247\,(1/Y) + 0.17 + 0.039\,L/Y - 0.26\,W/Y$$
$$(111)\qquad\qquad(0.09)\quad(0.016)\qquad\quad(0.15)$$
$$R = 0.35$$

At low income levels, the marginal propensity to save is almost negligible, the point estimate being only 0.0066. Above £1,000 this statistic jumps to 0.17.

Some words of elaboration about the transient elements in the lower tail of the income distribution are perhaps relevant. To a large extent, such observations are the result of temporary ill fortune, and people in such circumstances will frequently behave according to the income variability hypothesis. However, Katona[1] pointed out in his earlier discussion of income variability that there are differences in reaction to income increase and income decrease. People tend to react systematically to income decreases according to whether they are temporary or permanent, but in the case of income increass the results are more random. The theories under review are deficient in not building in adequately the asymmetry between decrease and income increase.

Modigliani's earlier theory stated that savings depended on the current income level and the highest previous level attained. While there is much to criticise in this over-simplified theory, it does have the desirable feature of introducing an asymmetry between increases and decreases of income. In fact, a key to the understanding of much of middle class savings in Britain to-day, especially in advanced age groups, is a resistance against a decrease in pre-war standards of living. With qualifications as to the time limits involved, it may be a universal phenomenon that people always resist *decreases* in consumption.

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[1] Katona, *op. cit.*

AN EVALUATION OF THE TESTS

By JAMES TOBIN and HAROLD WATTS

Dr. Fisher and the Oxford Institute of Statistics are to be congratulated for undertaking the important and formidable task of subjecting theories of economic behaviour to quantitative empirical test. The task is especially difficult because Fisher's investigation is the first encounter between the consumption theories of Friedman and of Modigliani and Brumberg, on the one hand, and the statistical raw material of the United Kingdom Savings Survey, on the other. The theories were not formulated with an eye to the kind of financial, economic, and demographic data collected in a single sample survey of households. The surveys were not designed to answer the kinds of questions raised in the theories. Fisher has done a heroic job of translation between the concepts and variables of the theories and the measures reported in the Survey. It reflects no lack of effort and ingenuity on his part that the results of his tests of the theories are inconclusive.

In general, those implications of Friedman and Modigliani-Brumberg which the Oxford data seem to confirm are propositions that their theories share with many other and prior theories of saving. The central propositions that differentiate the Friedman and Modigliani-Brumberg theories from others are not confirmed by Fisher's tests. Some of these propositions are not tested at all; for others, tests are attempted, but the results are inconclusive or negative.

As an example of the undifferentiated propositions that Fisher finds confirmed by the data, consider the effect of liquid asset holding on current saving. It is a commonplace of most post-war discussion of the saving function to expect that, given the level of (measured) income and other circumstances, saving will be negatively related to liquid asset holdings. By a complicated argument (pp. 247–249), Fisher finds that this expectation is also implied by Friedman's theory. Even when the data confirm this expectation, they provide no basis for choosing between Friedman's hypothesis and alternatives.

A second and related example is provided by the positive effects of income variability on the *average* propensity to save. Fisher finds the evidence that occupational groups with more variable incomes save a larger proportion of their incomes the most striking confirmation of Friedman's theory. This proposition is, however, an incidental rather than a central part of Friedman's theory. The plausible notion that households with volatile incomes will endeavour to provide themselves with contingency reserves can be accepted even if one is skeptical of the more distinctive propositions of the Friedman and Modigliani-Brumberg theories.

However, Fisher's interpretation and test of the contingency reserve hypothesis are not satisfactory. Income-variability is not measured directly in the Survey; hence it must be inferred from occupational category. As

Fisher himself notes, there is independent reason to believe that Self-employed are subject to income instability but no reason to assume this of Managers, who also show a high average propensity to save. Moreover, the contingency reserve hypothesis predicts that income variability will lead to a higher propensity to save not for everyone but only for households who have not already accumulated reserves. Thus, according to the hypothesis, the negative effects of liquid asset holdings on current saving should be more pronounced for variable-income occupations than for stable-income occupations. The coefficients shown in Appendix A do not conform to this prediction. But this may well be due to the fact that, for self-employed, the assets of the business or profession are as important a reserve as the liquid wealth measured in the Survey. Without data on those assets, and without evidence that the high saving ratio and low liquidity ratio observed for self-employed age 55–64 are not oddities of the sample, it would be hazardous to accept Fisher's *ad hoc* explanation that the high propensity to save observed for this group is due to an increase in income instability because of 'declining resilience' (p. 233).

A third instance of the confirmation of tangential components of the theories is the general success of Fisher's own modification to allow for family size. This is a definite theoretical improvement, and something of the kind must be done before the theories can be effectively applied to survey data. It should not, however, be so readily assumed that the relation of saving to family size is linear. It is certainly difficult to generalize about the effects of the present and future requirements of an additional member of the family on present saving, but it is by no means obvious that present consumption will rise with each additional member of the family, much less that it will go up by a constant amount. Some preliminary results of research in progress here on United States survey data indicate that families with one or two children save less than families with three or more children and that these larger families save almost as much or more than childless couples.

One of the distinctive and central propositions of the two theories under test is that the average propensity to save is independent of the absolute level of (permanent) income. This proposition follows from assumptions III and IV (p. 219) of Modigliani-Brumberg and the corresponding assumption of Friedman (p. 227) that initial wealth be kept intact over the life time, neither increased nor decreased. In contrast, the Keynesian theory of saving behaviour, and other theories, would predict a greater propensity to save *ceteris paribus* from high lifetime incomes than from low. Fisher's tests shed no light on choice between these alternatives. The high propensities to save exhibited by Managers might be interpreted favourably to a Keynesian hypothesis, and Fisher's reasons for dismissing this interpretation (p. 235) are not convincing.

A second major and distinctive feature of the Friedman and Modigliani-Brumberg hypotheses is that marginal propensity to save from transitory income is unity (Friedman) or very nearly unity (Modigliani-Brumberg—

set $\partial Y^e/\partial Y$ equal to zero in equation 2.5, p. 221). Empirical confirmation of this assumption is crucial to Friedman's interpretation of cross-section consumption-income regressions. As Fisher correctly notes (p. 240), it is circular to regard the ratio of the apparent cross-section marginal propensity to consume to the average propensity to consume as a measure of income variability without independent data on income variability.

If the marginal propensity to save from transitory income is not one but $1 - k'$, then the least-square regression coefficient a of saving on measured income for a cross-section of households is:

$$a = 1 - kP_y - k'(1 - P_y)$$

where, as in Friedman's theory, k is the marginal (and average) propensity to consume from permanent income and P_y is the proportion of variation in measured income over the cross-section due to variation of permanent income. Unless the value of k' is known and is different from k, an estimate of the regression coefficient a gives no indication of the magnitude of P_y, even if a good estimate of k is available from other measures.

If, as the theories under test allege, k' is zero or very small and if, in addition, P_y can be made small by classifying households into groups each of which is homogeneous with respect to permanent income, the coefficient a should approach one. This, indeed, is one of the principal tests on which Friedman himself relies. Fisher's estimates of the coefficient of saving on income show no striking tendency to approach unity as the cells are made more homogeneous; some become smaller, indeed even negative, rather than larger. In Fisher's investigation, classification by occupation and age may be regarded as an attempt to obtain groups with greater homogeneity of permanent income. Compare the coefficients of Y in the regressions for All Occupational Groups (Appendix A, p. 270) with the coefficients for the corresponding age groups for the separate occupations (pp. 265–269). Or compare the coefficient of Y in a regression for all age groups combined with the regressions for the separate age categories.

A central proposition in the Modigliani-Brumberg theory is that the marginal propensities to consume from income and from assets depend on age (except in the perfectly stationary case where the life-time income stream is correctly anticipated and an initial plan is exactly followed). Fisher (p. 256) finds no evidence of such a relationship.

In view of these results, it is difficult to find support for Fisher's conclusion (p. 262) that 'the really central implications of the Modigliani and Brumberg and Friedman theories do seem to survive these tests.' Some implications of these theories do survive some tests, but they are not the central implications.

We turn now to comments on the statistical methods employed in the investigation.

1. Fisher's worries about multicollinearity seem to be misplaced. The existence of correlation among two independent variables is no reason for excluding one of them from the regression, unless the correlation is perfect

or unless there are random errors in the 'independent' variables. In the latter case, the regression model is not an appropriate technique. Thus Fisher's reasons for avoiding non-linear forms for age and family size are not convincing. It is true that if, for example, X and X^2 are correlated, this correlation will increase the standard errors of the separate coefficients of these 'two' variables. But it is the fit of the parabola as a whole rather than the errors of the separate coefficients that matters. Furthermore, Fisher is surely wrong in his judgment (p. 217) that the failure of linear forms to give a good fit 'already ensures that complicated polynomial expressions will not advance matters'. Points falling symmetrically on a U-shaped parabola would give no significant linear relationship, but would show a perfect fit if a second-order term were added.

2. Fisher's reasons for rejecting the fears about heteroscedasticity that initially led him to experiment with S/Y as dependent variable are far from clear. The low values of R^2 in the S/Y equations (1.12) and (1.13), p. 213, do not constitute a valid reason, since it is not to be expected that correcting for heteroscedasticity improves the fit. Table 2.11, p. 253, shows standard deviations of residuals by age-occupation cells. But since the initial problem was the relation of the standard deviation to income and assets (not to age and occupation), it is hard to see why this is adequate justification.

3. In general, Fisher pays too much attention to the value of R^2. Without simultaneous consideration of number of degrees of freedom, which Fisher never includes in his tables, the value of R^2 is no guide to significance. Incidentally, the reader should be kept better posted on the use of asterisks and other devices to indicate statistical significance in the Tables.

4. Finally, in our opinion, Fisher unnecessarily cripples his analysis by his decision (p. 203) not to regard the variation of saving over the entire sample as the variability to be explained. Instead, he takes for granted that much of the variation can be explained by classification, and takes as his problem the separate explanation of the variation within each cell of the classification. Thus he does not ascertain by test, but rather assumes, significant interaction between his occupation-age classification and his scaled variables, income, liquid asset holdings, and family size. Many of the propositions that concern Fisher involve differences in the coefficients of these scaled variables between occupation-age groups; but no tests are really made of the significance of the differences that appear.

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SOME CONDITIONS FOR PROGRESS IN THE STUDY OF SAVINGS

By IRWIN FRIEND

Dr. Fisher's paper makes available a wealth of new data on the saving behaviour of different groups of individuals, based on an analysis of the results of the 1953 British survey. However, I shall limit my remarks to the core of his paper which is devoted to detailed testing of two recent theories of saving propounded by Professors Modigliani and Friedman.

My overall reaction to these tests is that they do not provide strong corroborative evidence for the two theories, both of which I shall loosely refer to as ' permanent-relative income ' hypotheses. First, much of the detailed analysis does not seem to support these theories—particularly the Modigliani version—though this part of the analysis may be, as Fisher suggests, peripheral to the basic implications of the theories. Second, where more critical tests of these theories have been carried out, they have not been made in such a manner as to permit a choice between permanent-relative income and various other hypotheses.

In connection with the first point, Fisher notes (p. 261) that 'This failure of the Modigliani theory to elicit strong support for those aspects that concern age and family size does not, we feel, destroy the theory. It means that the outer fortifications are impaired but not necessarily the citadel itself.' The ' citadel ' according to Fisher is the assumption that ' the proportion of an individual's total resources that he plans to devote to consumption in any year of his life is determined only by his tastes and not by the size of his resources.' It seems legitimate to query why so much effort has been spent on the ' outer fortifications '. This question is all the more germane since, in the Friedman theory also, a basic assumption is that the proportion of consumption to discounted expected income is independent of the level of such income. (The second key assumption in the Friedman theory is that transitory components of consumption and income are uncorrelated.)

Turning to the second and much more important point, the basic corroborative evidence adduced to support the ' permanent-relative income ' hypotheses might be explained fully as well and perhaps better by other hypotheses for which these recent theories are offered as substitutes—notably various ' absolute income ' theories which in their simplest version have attempted to explain current saving in terms of the level of current income, assets, etc., though non-current values of these variables have also been introduced. There would seem to be two critical questions in choosing between the ' permanent-relative ' and ' current-absolute ' or other absolute income theories. To what extent is current saving determined by current resources alone, and to what extent is it necessary to introduce past and future resources? To what extent is the level of saving a linear homogeneous function of the level of resources? If current resources alone—because of the influence of uncertainty or because it is a convenient proxy variable—can explain about as much of current saving behaviour as resources over some longer time span, there is not much point in introducing past and future resources. If the ratio of saving to resources is approximately constant for different resource groups, there is not much point in introducing more complicated functional relationships. However, the evidence in the paper which is taken to support the ' permanent-relative income ' hypotheses—presumably implying a planning horizon of many years (at least in the Modigliani version) and making the assumption that the ratio of saving to resources is independent of the level of resources—does not really meet these questions squarely.

Specifically, Fisher finds his basic support for the ' permanent-relative income ' hypotheses, particularly for the Friedman version, from these types of evidence: presumably most important, the fact that current saving/income ratios for different occupational groups vary generally with income instability (p. 233 ff.); and also that consumption/liquid asset ratios initially vary directly with current income and then inversely (p. 250). This does not seem to me to be a convincing bill of particulars. It is my impression that the differences among the current saving/income ratios of the various occupational groups might be explained about as well by their current level of income, except for the difference between managers and self-employed; and it is possible that the differences in consumption/liquid asset ratios of the various income groups might be explained in a similar manner—*i.e.*, by the relation of consumption to current income or to total current resources with consumption rising more slowly than income and income more slowly than liquid assets—once the lowest income groups, apparently reflecting heavily the ' retired and unoccupied ', are eliminated.

In connection with the first of these points, it may be of interest to refer to the U.S. urban 1950 saving/income ratios by occupation presented in a forthcoming paper carried out as part of the Wharton School-B.L.S. Study of Consumer Expenditures, Incomes and Savings.[1] The following table presents these data:

TABLE I. *Saving/Income Ratios, by Occupation: U.S. Urban, 1950*

Occupation	*Average after-tax income* (1)	*Saving/income ratio*	
		Excluding durables (2)	*Including durables** (3)
Self-employed	$6,270	14.4	24.8
Salaried prof., managers, officials	6,110	9.8	21.2
Clerical and sales	4,349	2.9	14.2
Skilled wage earners	4,431	1.9	16.1
Semi-skilled	3,758	1.6	14.3
Unskilled	2,731	— .9	9.2
Not gainfully employed	2,133	−12.0	— 2.5
All groups	4,147	4.3	15.9

Source: B.L.S.-Wharton data. Population weights were adjusted for under-reporting of high income units. * Consists of purchases of automobiles, television sets, radios, musical instruments, furniture and household equipment.

[1] Irwin Friend, and Irving B. Kravis, ' Entrepreneurial Income, Saving and Investment ', *American Economic Review*, June, 1957.

To quote from that paper:

> These data do not appear to support the permanent income hypothesis.[1] Aside from the entrepreneurial group, there seems to be little basis for believing that the ratio of transient to permanent components of income and consumption differed substantially from one occupational group to another. Yet if the occupations are arrayed in order of decreasing incomes, the saving-income ratios decline in a fairly regular fashion. . . . One possible way of reconciling these data with the Friedman hypothesis, which does not appear to us to be convincing, would be to argue that the lower-income occupations consist of many persons who are on their way up to higher-income occupations. Because of the inclusion of these persons, it might then be claimed, the average spending of the group is raised by the optimistic expectations of the potential climbers.

The essential point is that it is necessary to look for more critical tests to differentiate among competing theories. In the Wharton School-B.L.S. Study of Consumer Expenditures a number of different approaches are being taken to test the relative importance for saving behaviour of current *vs.* non-current or ' permanent ' income, of absolute *vs.* relative income, etc. Thus U.S. urban consumer units in 1950 have been classified on the basis of the stability of their income as measured by the change in their income from the preceding year and anticipated change in the following year to analyze the saving and consumption behaviour of persons with ' stable ' *vs.* ' unstable ' income, and the market value of homes and rent are being used as additional proxy variables for ' permanent ' income status. Moreover, the influence of windfall income and capital gains on consumption patterns is being studied—*e.g.*, the effect of the National Service Life Insurance dividend in early 1950, which according to the permanent income hypotheses should have been preponderantly saved. The consumption and saving patterns of high, medium, and low savers in different income groups (*i.e.*, the distribution of total consumption among major goods and services and of total saving among major assets and liabilities) are being analyzed to determine, for example, whether the consumption patterns of high savers—who according to the permanent income hypothesis tend to be people with temporarily high (or unusually variable) income—are closer to that of medium savers in the same current income group or medium savers in a low income group of the same total consumption.[2] Finally, and potentially most useful, are tests of these different hypotheses *vs.* data on income and expenditures from a continuous sample of respondents, and the analysis of such data on food and automobile expenditures for several recent years (based on two continuous panels of about 3,000 and 4,500 consumer units obtained from market research agencies) has already begun.

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[1] It might be noted, in view of Fisher's emphasis on occupation data by age groups, that for each occupation the average income for all age groups combined can be considered to give a good indication of permanent income status.

[2] This test would show whether there are smaller differences in spending patterns for high, medium and low savers if total consumption (as proxy for permanent income status) rather than current income is held constant. It assumes that the transitory component of consumption does not overshadow that in income.

Though most of this analysis is still in a preliminary stage, it may be useful to present a direct comparison of the percentage distribution of high, medium, and low savers within income classes for all U.S. urban consumer units in 1950 divided into two groups; those with a three-year span of relatively constant income who indicated they had ' about the same ' income in 1950 as in 1949 and anticipated ' about the same ' income in 1951 (Table II A), and those with more variable income (Table II B).[1] Under either the Friedman or Modigliani hypotheses, it would be expected that the cross-section average propensities to save would be much more constant from one income class to another for the A group of consumer units in Table II than for the B group. In contrast, for the upper current income groups there is very little difference between the distributions of consumers by size of saving (and therefore between the average propensities) when those with stable income over a three year span are segregated from other consumers.[2] This would seem to constitute fairly important evidence against the permanent income and related theories since a high proportion of total net saving is concentrated in the upper income groups. On the other hand, for the three lowest income groups, unusually low saving relative to current income is associated with either an actual or expected change in income, which is consistent with these theories. As a whole, the table does not seem to lend much support to the superiority of permanent income and related theories over other hypotheses.

These findings seem to be attributable only in part to differences in the ' mix ' of consumer units with a relatively constant income over the three year span as compared with other consumers. Thus, when salaried persons (*i.e.* not self-employed) were segregated from entrepreneurs, and age, family size and region were all held constant, the results are only moderately improved from the viewpoint of the permanent income theories. Since savings position in this analysis excludes personal insurance (simply because the data were most readily available in this fashion) and excludes investment in durable goods other than housing, it should be noted that a supplementary analysis of saving including insurance and durable goods does not indicate any change in the above findings.

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[1] This comparison of the percentage distribution of high, medium, and low savers within income classes is used instead of the more direct comparison of average saving or consumption propensities within income classes simply because of the earlier availability of the former data. The latter, direct comparison, as well as a number of other tests of the ' permanent-relative income ' hypotheses, will be presented in a forthcoming paper (Irwin Friend and Irving B. Kravis, ' Consumption Patterns and Permanent Income ', *American Economic Review*, Papers and Proceedings, May 1957). It may be noted that the slope of consumption on income for all families in 1950 turned out to be insignificantly different from that for the three-year constant income families. (The figures are .690 and .681 respectively for all income groups, and .818 and .815 for income groups from $1,000 to $10,000, based on frequency weighted arithmetic regressions.)

[2] It should be noted that the average income in open-end class A is somewhat higher than in open-end class B.

TABLE II

Percentage Distribution of U.S. Urban Consumer Units in 1950 by Savings Position within Income Class

A. Constant Income Units[1]

Income after taxes ($)	*Sample frequency*	*Savings Position*[2] *Under −25%* (%)	*−25% to −5%* (%)	*−5% to 5%* (%)	*5% to 25%* (%)	*Over 25%* (%)	*Total* (%)
Under 1,000	352	35.2	10.2	47.0	6.2	1.4	100.0
1,000—1,999	547	14.2	17.0	50.1	14.9	3.8	100.0
2,000—2,999	632	11.5	23.2	41.8	19.6	3.9	100.0
3,000—3,999	680	13.2	22.3	30.9	28.6	5.0	100.0
4,000—4,999	460	13.0	19.7	28.5	32.1	6.7	100.0
5,000—5,999	282	9.2	23.0	27.6	34.2	6.0	100.0
6,000—7,499	177	6.2	17.5	27.1	37.4	11.8	100.0
7,500—9,999	97	5.1	15.4	26.8	34.2	18.5	100.0
10,000 and over	67	1.4	14.9	20.8	29.9	33.0	100.0
All	3,294	14.2	19.4	36.8	23.8	5.8	100.0

B. Non-Constant Income Units

Income after taxes ($)	*Sample frequency*	*Savings Position*[2] *Under −25%* (%)	*−25% to −5%* (%)	*−5% to 5%* (%)	*5% to 25%* (%)	*Over 25%* (%)	*Total* (%)
Under 1,000	419	55.0	10.0	30.1	2.8	2.1	100.0
1,000—1,999	983	25.6	20.4	38.3	12.8	2.9	100.0
2,000—2,999	1,713	17.5	29.6	32.5	16.9	3.5	100.0
3,000—3,999	2,305	15.4	27.3	29.2	24.2	3.9	100.0
4,000—4,999	1,654	13.0	25.5	28.0	28.5	5.0	100.0
5,000—5,999	931	11.7	21.5	27.0	33.2	6.6	100.0
6,000—7,499	627	8.7	22.9	25.8	32.4	10.2	100.0
7,500—9,999	333	6.3	15.3	24.6	35.5	18.3	100.0
10,000 and over	230	1.7	8.2	23.0	31.3	35.8	100.0
All	9,195	16.8	24.1	29.9	23.4	5.8	100.0

Source: B.L.S.-Wharton data. Unlike the date in Table I, these are unadjusted sample results.

[1] Units with about the same income in 1950 as in 1949 and expecting about the same income in 1951.

[2] Ratio of savings, exclusive of personal insurance and consumer durables, to income after taxes.

There is one final reservation I have both to Fisher's paper and to all similar analyses of survey data. He points out (p. 205) that 'the survey estimates of incomes and assets when magnified to national dimensions are appreciably lower than alternative estimates, the main direct operating factor being the non-cooperation of many of those in the higher wealth classes and a tendency by those in these groups who did respond toward understatement of their holdings.' The significance of this statement is underlined by the apparent finding that in the sub-sample analysed by Fisher only .0033 of aggregate income was saved in forms other than consumer durables. The U.S. survey data also indicate very substantial understatement of saving.

If the ratio of upper income saving to income is even higher than now shown by survey data, it would be all the more difficult to explain, in terms of the ' permanent ' income and related theories, the very much higher saving ratios of the top income groups classified by current incomes by adducing astronomical saving ratios for persons temporarily in the top income groups superimposed on average ratios for those permanently in these groups. However, without knowledge of how the survey understatement of saving is distributed, all statements about the saving behaviour of different groups are suspect. My guess is that an adjustment of the survey data for their major deficiencies—a process in which I am now engaged—will serve to raise the saving/income ratio for upper incomes more than for lower incomes, through the proportion of total personal saving attributed to the upper income groups may be lowered.

Moreover, it should be emphasized that survey data indicate that a very high proportion of total personal saving is done by the upper income groups in general and entrepreneurs in particular. Since it is the variability in total personal saving that we are basically interested in explaining, rather than saving at income x, age y, and family size z, it is my feeling that major progress in explaining personal saving behaviour will depend on improving the available data on saving of the upper income groups and entrepreneurs, and utilizing theory which both addresses itself specifically to, and is able to explain the available data in, these areas.

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THE DANGER OF OVER-SIMPLIFICATION

By J. D. SARGAN

We must all be grateful for the skill and thoroughness with which Dr. Fisher has considered the results of the Oxford Savings Survey in the light of the Modigliani and Friedman theories of consumption behaviour. These comments are merely a series of rather disjointed criticisms arranged in the order in which they arose in reading through the articles, and they must not be thought to subtract from the tribute that is due to Dr. Fisher.

This investigation raises very clearly the problem of the relationship between regression equations estimated from cross-section data and structural relationships. One of the fundamental problems here is that there may be more than one structural relationship connecting the variables being studied. This leads to much the same kind of problem as in the statistical analysis of the inter-relationships between economic time series. The problem arises in studying savings because three of the variables considered, current savings, total assets, and income are related in several ways. There is first a relationship explaining how current savings are determined, the important variables whose effects could be detected from cross-section data being income past and present, and total assets. There is secondly a relationship saying that total assets are the sum of accumulated past savings. Previous investigators have stressed that attitudes to savings are highly idiosyncratic, and can be expected to persist, so that high savings in the past are likely to be accompanied by high current savings. Thus, in cross-section data this relationship will take the form of a positive relationship between total assets and current savings. There is thirdly the relationship explaining how current income is determined and, for the self-employed at least, an important variable may be total assets. In addition, for the self-employed, if savings are largely invested in the business, the savings schedule may get mixed up with the marginal efficiency schedule, since high profits may provide an incentive for additional investment.

All this means that the distribution of these variables obtained from a single cross-section survey is determined in a rather complicated way from the dynamic structural relationships, and this may be further complicated, if ' liquid assets ' must be substituted as a proxy for ' total assets '. If a suitable linear dynamic model were used to represent each individual's behaviour over time, and if different individuals' models were assumed to differ in their parameters, the cross section distribution of the economic variables might be deduced from the distribution of the parameters of the relationships. The converse deduction of the distribution of the parameters from the cross-section distributions of the economic variables is not equally easy, and without a very specialised model is usually impossible. And the problem is even worse if it is assumed with Friedman that the sample variables differ from the ideal variables because of differences of definition, or because there are errors in eliciting the true values of the variables from the members of the sample, or because of errors in a later stage of analysis of the sample (and judging from the list of extreme members of this sample such errors are not absent here). Indeed, it might be argued that the complexity of the problem makes it reasonable to assume that in this case the study of the movement of macro-economic time series can contribute more to the explanation of cross-section data than *vice versa.*

It is possible to argue that normal regression relationships will provide the best forecasts of saving, but only in the sense that if another sample of households had been chosen in 1953 in the same way that the actual sample was chosen, then the best way of estimating the savings of any member of this second sample from the other information obtained about this member of the sample would be to use the regression relationship. This, however, does not mean that the regression relationship used in the normal way can be used to forecast, for example, the effect of a change in government tax policy on savings.

In these circumstances it is tempting to seize on any suggested simplification in the form of the structural relationships, and both the Modigliani and the Friedman model have the point in common that they suggest that ultimately consumption is proportional to income. It seems likely that the acceptance of this kind of limitation on the model has been suggested by the observed constant proportion of savings to the national income in the U.S.A. over the last eighty years. But it is not necessarily true that a simple regularity demands a fundamental explanation. Long term statistics give several examples of similar regularities where no fundamental explanation seems possible, and where it seems most reasonable to conclude that this is just the way the trend happened to be. And in this case there can be cited a number of important long term trend factors tending to decrease saving. Thus, the relative increase in taxation and at the same time the increase in insurance and in government sponsored relief schemes together with increases in estate duties all tend to discourage savings as a proportion of the national income. At the same time, technological developments particularly in consumer durables alter the whole nature of the utility function, increase the range of consumer possibilities and may be assumed to have encouraged consumption. Disposable income has also become perceptibly more equally distributed. But more important than these is the decline of the nineteenth century idea that abstinence is morally good and that thrift is a prime virtue of the serious citizen, combined with the growth of advertising as a serious and scientific business of persuading the consumer to consume. It would need a considerable opposing force to combat all these factors, and this is surely only provided by the effect of the general rise in real incomes. Indeed, the latest figures for the U.S.A. and the U.K. seem almost to justify the speculation that the proportion of income saved is rising. It seems, therefore, that the positive grounds for adopting the assumption that for the individual consumer the proportion of income consumed is constant are not very strong.

Turning now to a more detailed criticism of the Modigliani model, the model is interesting primarily because it directs attention to the effect of age on savings. It does this at the expense of a considerable unreality in its assumptions. The most obvious criticism is of the assumption that at all ages the saver is saving purely to finance consumption after retirement. It seems unlikely from introspection and from enquiries of a small sample of friends that an explanation of savings at ages of less than 40 which concentrates on savings for retirement is going to be very successful. People indeed differ greatly in their motives for saving, and in their savings behaviour, and an attempt to rationalise their behaviour too completely is necessarily unrealistic. But a rationalisation which stresses remaining lifespan as a main determinant will probably give poor results in the lower age groups. Indeed Modigliani's main points may be considered more realistically covered by the Friedman theory, if it is realised that from its definition permanent income is just the constant level at which consumption could be maintained throughout the remaining life of the individual if he wishes to leave neither debts nor assets at death. Thus, permanent income is less than current income if the individual is expecting constant income throughout his working life and zero income on retirement. But it would seem, if we are to be more realistic, that the rate of interest used to reckon V_t (in equation 2.21) should be large so as to take account of risk factors, so that the expectation of income in 20 years time should have little effect on current saving. Similarly, if it is assumed that the young expect a growing income both models would explain the effects of this in much the same way, the Modigliani by saying that Y_e is greater than Y, and the Friedman by saying that permanent income is greater than current income as normally defined. At greater ages, the Modigliani model may be better in practice in giving a simpler approximation to the effects of saving for retirement.

In considering Fisher's modifications of the Modigliani model by taking account of differences in family size, it may be noted that this consideration is more difficult to formulate in a Friedman type model so that the Modigliani model may be useful to indicate the direction of the effects of family size even if the absolute value of the effects is probably overstated. However, the model becomes particularly unrealistic if it indicates that the income unit has negative assets at or near the greatest family size, and presumably must be modified to assume that a lower consumption level is maintained before this point than later. Fisher's adaptation of the Modigliani model that he considers appropriate to cases where income is growing at a constant rate suffers from the defect that it is not easy to discover a utility function that would lead to consumer behaviour of the type envisaged. There is indeed no reason why in a community where real incomes are rising each income unit should not keep its consumption constant (except for the practical difficulty of borrowing in the earlier stages). Even so aggregate consumption would still rise at the same ate as aggregate income. This model would however be unrealistic in assuming that people can accurately foresee the future so as to choose the appropriate level of consumption. The type of behaviour in which each income unit keeps its consumption proportional to income is very much better explained by a Friedman model. The use of the Modigliani model involves the semi-contradiction that on the one hand the consumer is supposed to foresee the future accurately but on the other hand he has not the sense to stabilise his consumption level over time.

Finally, in criticism of the Modigliani model it may be noted that it does not imply the constancy of the proportion of the national income saved over time. If aggregate saving is considered the substantial cancellation between savings before retirement and dissavings after retirement postulated by this model would ensure that aggregate savings depends strongly on the rate of change of real incomes, prices, number of families, average size of families, length of working life, and expectation of life. There would be a tendency for the proportion of income saved to be constant only if all these rates of change are constant or if the effects of changes in them cancel.

Turning now to the Friedman theory of consumption, there is a basic assumption which is fundamental to this theory which must be challenged. This is that the utility function can be assumed to be homogeneous in the value of consumption at different dates. For this to be true for all possible sets of prices for the different commodities it would be necessary for the utility function to be completely homogeneous in the quantities of all the different goods consumed. And, if this were true, then a change in the level of income would produce proportionate changes in the consumption of all commodities. This is so contrary to common sense and the results of empirical research that the assumption of complete homogeneity must be dismissed. It follows that Friedman's basic assumption of the homogeneity of the utility function considered as a function of the total value of consumption at different periods could only hold true for certain special sets of prices. And it seems *a priori* rather unlikely that one of these special sets of prices should happen to coincide with the actual or expected market prices.

The real advance made by Friedman is the recognition of the importance of the distinction between transitory and permanent income components (the transitory components can be thought of as including measurement and analysis errors). However, in using this idea the important assumption is made that transitory components of income are not cross-section correlated. Theoretically this could be true, but in practice it seems probable that temporary fluctuations in income might be expected to affect consumption and this would lead to a positive correlation. In particular, errors in income measurement would lead to an equal error in the estimation of consumption so that this would certainly lead to a positive correlation. To consider this problem further it is necessary to consider what factors determine the size of the transitory components as defined by Friedman. From the definition of permanent income it is clear that it depends upon expectations of future income and it seems reasonable to suppose that these expectations depend upon experience of past income, apart from more definite contractual information. It would be possible to assume that the rate of change of income is extrapolated, but it seems more in accord with the results of econometric studies to assume that the extrapolation is based on the average level of income over some period in the past. In this case, permanent income would be related to average income over some period in the past, and if this period were short and recent the difference between permanent income and current income would be small, but if the period were rather long or not recent then differences between permanent income and current income would be larger. From econometric analysis in this country and in the U.S.A. it has been found that consumption is well explained by relating it to current real wages and to non-wage real income with a lag of about one year. This may be the result either of non-wage consumption being fixed with regard to average income over a period of up to two years, or simply because of a longer lag between income and consumption due to inertia (which can be given more play at higher income levels, since to put it crudely the marginal utilities involved are probably smaller) or to it taking longer to determine income, particularly for the self-employed. This suggests that differences between permanent income and current income would be lower for wage-earners tban for non-wage-earners, even if the two types of income are equally variable over time. Similarly, if people differ in the rate at which they discount future incomes over time, the differences between permanent incomes and current incomes would be lowest for those whose rate of interest is largest. Now according to Friedman the rate of interest to be used for discounting is an objective rate, representative of the actual market rates of interest, but it seems a convenient way of making the theory more realistic to think of it as a subjective rate, perhaps related to the market rate, but also representing risk factors, and likely to be different for different social groups. It might be argued that the wage-earner, protected to some extent against misfortune by the welfare state, may feel that he cannot afford the luxury of worrying about the future, and so discounts his expectations of future income heavily in deciding his current expenditure. The non-wage-earner through his early environment, and influenced by the different social attitudes of his class, might be expected to discount his future income at a lower effective rate of interest. This again might cause the non-wage-earner to have larger transitory components than the wage-earner, without there being any implication that the non-wage incomes are more variable than wages in their fluctuation over time. It is also possible that lack of information about changes in currency holdings have had the effect of concealing some transitory savings by wage-earners which would affect this occupational group more, especially since they do not usually have commercial bank deposits.

Turning now from a discussion of the models to the results of Fisher's analysis, it is clear that there are considerable advantages in separating the different occupational groups. The data when separated in this way do appear to be difficult to explain by a general equation of the form 1.13, and the failure of the general equation to explain the facts does seem to indicate that a Keynesian explanation of the differences, in terms of increasing marginal propensities to save due simply to higher income, is not adequate. However, there are alternatives to the Friedman explanation of the differences favoured by Fisher. The higher saving of the self-employed may reflect the fact that the effective rate of interest yielded by the average firm is higher than the market rate that can be obtained by the non-self-employed, and this may raise the marginal propensity to save of the self-employed. Furthermore, high profits may be correlated with high profitability of new investment, and this will raise the regression coefficient above what it would be in the absence of this factor. For the managers the difference may be a class difference, reflecting differences in upbringing and social attitude, in particular because of a desire to avoid falling, in times of sickness or unemployment, to levels of consumption which are regarded as inappropriate to the manager's social status. The discussion of the differences between age groups is more difficult. Here the temptation is to regard the data as revealing the life history of some kind of average or representative member of each occupational group. However, it is clear from a study of age distributions and from common sense that people move between these occupational groups in a more or less systematic way. Managers are recruited from clerical and sales workers at all ages, but certainly in some volume up to the age of 45. Similarly people enter the self-employed category at all ages, and indeed there is a tendency for some people to save so as to retire into self-employment. On the other hand, there is a tendency for the unsuccessful to leave the ranks of the managers and the self-employed, and conversely the successful in these groups often retire early, sometimes, in the case of the self-employed, in order to allow their sons to take over. Nor is it true that these changes of occupation are unconnected with savings habits. The new entrant to self-employment is likely to have been a heavy saver in the past to accumulate wealth to purchase the business, or if he is starting a new firm to carry him over an initial period when the firm may make losses. Similarly, if the entrepreneur is starting a new firm he may have more liquid assets than the old-established entrepreneur, a lower income, and a tendency to dissave. Moreover, since managers probably come mostly from the clerical and sales group, it is probable that this occupation group at lower ages can be split into two strata, an upper stratum consisting of the potential managers, and a lower stratum consisting of the rest. It is probable that the upper stratum differs significantly from the lower stratum in savings attitudes. Finally, it may be noted that the lowest age group includes a large number of women who marry before they reach the higher age groups. Their attitude to savings is likely to be different from that of men, and this makes any interpretation of the data for the lowest age groups difficult, particularly for the clerical and sales group. It may be observed also that since the data were obtained only eight years after the end of a major war the length of time for which the members of each occupation had been in each group was probably abnormal, especially in the middle age groups.

All this suggests that Fisher's procedure in his last section needs close scrutiny. Thus, it may be the best we can do to estimate that when the managers at present in the 18–24 age group are in the 25–34 age group their average real incomes will be 16 per cent or 22 per cent greater than for the average for the managers at present in this group, although this makes assumptions about the homogeneity of the new entrants with the existing members of the group which may be invalid. But to assume that their consumption will then be an equal percentage greater than the consumption of the current members of the group is very arbitrary. After all it is not being assumed that their own incomes have increased by this stated percentage. It would be more reasonable to use the sample regression equation for the 25–34 age group to estimate what would happen to average consumption, if income increased by the given percentage. If the result of this differed from the suggested percentage increase then so much the worse for the rather arbitrary initial assumption. Indeed, Fisher's procedure can be regarded as the construction of a hypothetical sample based upon an arbitrary assumption. If the regression coefficients obtained by an analysis of this hypothetical sample differ significantly from those obtained by an analysis of the actual sample this can only be because the sample data contradict the arbitrary assumption. In any case, the procedure only distorts the regression coefficients from what are in at least some cases the optimum estimates. If, however, Fisher is concerned with the Friedman criticism of the regression coefficients as being affected by transitory components of income, it is clear that Fisher's revised regression coefficients are equally unreliable for this reason, and if he further accepts the Friedman assumption that consumption is proportional to income, it seems that the only reasonable solution is to discuss how the ratio of average saving to average income is related to age and family size.

Finally, looking at the data presented in Appendix A in a more realistic kind of way, it seems worthwhile to stress the importance of a more thorough consideration of the significance of the differences set out in these tables. Having regard to the non-normality of the distributions of the variables and to the large number of regression coefficients that are being compared there is a probability of order 1 per cent that one or two of these coefficients should differ from their population values by more than four times their standard error. Even so, there are clearly some significant differences between age groups, but it seems clear that no simple and general theory will explain them. This is obviously one of the fields where initially the more one investigates the more complicated it appears. It seems clear that to increase the proportion of the variance which is explicable further information and a fuller analysis are required. For example, an analysis by the sex of the head of the income unit might throw some light on the divergent behaviour of the 18—24 age group. Similarly it might be possible to devise a slightly better split by occupational group. Thus, it might be interesting to split the manual workers into skilled and unskilled groups, to separate out sales from minor clerical workers, and to allocate potential managers to the managerial group; to split the retired from the unoccupied and to separate the retired by the occupation group to which they used to belong. All this might not be worthwhile with the present size of sample but probably would be with a larger sample. Similarly, it might be interesting to split the sample by the type of district and the part of the country in which the informant lives.

Again, in any future enquiry there is much extra information that might be considered to be worth collecting. For example, it might be useful to obtain a brief life history from the respondent, covering occupation of parents, education, and any past change of occupation group or any expectation of such change. It would also possibly be worthwhile to explore attitudes to saving by asking respondents why they are saving or dissaving, and by exploring factors which might affect savings attitudes such as psychological type, religious affiliations, nationality, and perhaps parental attitude to savings. It may be objected that this would lead to such a complicated interview that satisfactory response would be unlikely. However an attitudinal survey in this country might be useful even without an attempt to measure savings, in at least providing some basis for a choice between a Modigliani type theory which assumes that all saving is for retirement and a Friedman type theory with its emphasis on risk and transitory factors. In this connection, in a savings enquiry the respondents might be asked whether they thought that their incomes and consumption had been normal in the year in question, and if not why. For the self-employed it might be of some interest before getting down to the questions which enable income to be calculated accurately, to ask the respondent to state what he thought his income had been offhand.

Finally, it seems fair to say that Fisher's investigation has been a very interesting experiment in studying abstract theories in the light of empirical cross-section data. If the result is only partially rewarding this can be wholly blamed upon the abstraction of the theories and the complexity of the real world. Fisher's investigation has been thorough and illuminating in its revelation of the complexity of the problem and in suggesting new lines for later investigation.

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A REPLY TO THE CRITICS[1]

By MALCOLM R. FISHER

At the outset, one point must be made clear. My paper was concerned with the screening of two hypotheses with the material yielded by a one-period cross-section survey alone. It was never thought that such a limited amount of evidence would provide in itself fully effective discrimination between these and alternative hypotheses. In particular, I should regard parallel time series and reinterview cross-section studies as essential for this purpose. All I do claim is that these hypotheses should be able to withstand tests that may be effected with a one-period sample. Reinterview surveys have been carried out at Oxford, but the results quoted in Klein's paper are the first I have seen. Three important pieces of information were neglected in my study. Firstly, information was sought and obtained from respondents on past income change. It was my impression from reinterview questionnaires, that I edited that this question was not adequately answered in terms of the survey concept of gross income. For this reason this item was neglected. Presumably, the same problem arises in respondents' answers concerning income expectations. It can be argued that it is surely what respondents regard as their income that matters. But the current level of income on the respondents' own definitions was not requested. Secondly, some information was obtained on windfalls. To the reason for their omission we shall return. Thirdly, some information was obtained on assets such as the current values of cars, property, and security holdings, but there was some doubt as to the reliability of this material. Moreover, through the number of ' don't know ' or imprecise answers to these questions we should have been forced to discard information from a number of respondents in groups where our degree of coverage was already small. And, less important, this information related to end of year, not beginning of year, valuations. For these reasons—perhaps, as Modigliani considers, wrongly—asset information other than about liquid holdings was ignored.

I share with my critics the view that these and alternative hypotheses should be tested as widely as possible and, as far as the Friedman hypothesis is concerned, this requires fuller information on income distributions. I would agree that the more critical tests must be made with data other than that available to me. But a case may be made for rejection or modification even at this level. Perhaps it is true, as Friend suggests, that my paper is more concerned with ' the outer fortifications '. But even here we may point to alternative explanations of phenomena to those implied, by say, the absolute income hypothesis.

Whereas my article provides in itself insufficient material to justify (or reject) under even most favourable conditions the conditional acceptance of these new hypotheses, it may, I think, be argued that my critics, drawing on their own studies in more varied fields, have produced their strongest weapons for attacking these hypotheses. These we shall study with care. But for balanced assessment it is only fair to warn the reader that he should reserve judgment at least until Friedman's forthcoming book has been digested. Both Modigliani and Friedman would query my credentials as counsel for their defence.

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[1] The writer has benefitted from discussions with Mr. M. J. Farrell on several points though the writer alone is responsible for views expressed. For economy in the case of joint authorship only the first-named is referred to. No discourtesy is implied.

Savings Hypotheses

The central feature of the Friedman hypothesis is that the permanent consumption/permanent income ratio is independent of permanent income but may depend on a number of other factors, *e.g.*, age, family size, interest rates, uncertainty, tastes, etc. Another major feature, though not indispensable, is the assumption that, on the average, transitory consumption and income are uncorrelated. The central feature of the Modigliani hypothesis is essentially similar and is stated in my assumption IV, p. 219. Some justification for preferring these to certain absolute income hypotheses is provided by the relative constancy of the saving/income ratios obtained from long period time-series studies in the U.S.A., though the case for them does not depend unequivocally on this evidence. If, as some critics claim, the central implications of these new hypotheses are somewhat elusive, the same may be said for the absolute income hypothesis. In fact, one is reminded of questions raised in the 'thirties as to the content of the classical theory of interest. Tobin now tells us that the (extended?) absolute income hypothesis states that the permanent (lifetime) consumption/permanent income ratio moves inversely with permanent income. With this I have no quarrel, but I am surprised to learn that the view that groups with more volatile incomes will save more in order to maintain their reserves/income ratio at a desired level is considered part of this hypothesis. A clear statement of this hypothesis is perhaps overdue.

Turning to the Modigliani hypothesis, Sargan makes some penetrating criticisms which bear upon one of my empirical suggestions in particular. The search for a behaviour relation which assumed a regularly progressive income over the earning span and yet preserved certain simple features of the stationary model led me to propose equation (2.8). Sargan justifiably attacks this as being at variance with familiar properties of utility functions under certainty. Institutional and borrowing difficulties alone stand in the way of a continued acceptance of assumption VI. Sargan also challenges an assumption concerning the utility function implied in the Friedman hypothesis—namely, that it is homogeneous in the volume of consumption at different dates. His strictures should apply likewise to the Modigliani hypothesis which makes an identical assumption. His point about its implications for individual commodities is answered in Chapter II of Friedman's monograph. The assumption itself is the one that permits the considerable break through of these hypotheses and, while of the ' let us assume individuals behave *as if* ' class, appears justified as a working rule until the hypothesis is substantially contradicted. One of the weaknesses of much empirical analysis relevant to the testing of theories has been an unwillingness to impose sufficiently restrictive initial assumptions on the functions, particularly with reference to the choice of functional forms. Often choice of forms has been made by letting the data dictate, as in Part I of my paper, and I certainly now feel that in an extreme form this is unsatisfactory. Interestingly enough, even with these severe simplifications it is still difficult to discriminate between hypotheses. Of course assumptions that are *a priori* implausible should not be employed. Some differences of emphasis between Sargan and myself do seem to stem from the question as to how ' realistic ' assumptions should be.

Hill raises the pertinent criticism that in the Modigliani model insufficient attention is given to changes in income that are entirely in conformity with original expectations and to their effect on the marginal propensities. Where I part from him is over his judgment that unexpected changes in income are not likely to be at all permanent. In my view, just such changes with expectation of continuity are very characteristic of the world we know. Hill's point is sound but, in my view, of secondary practical importance.

EMPIRICAL ASPECTS OF THE HYPOTHESES

Sargan and Klein are critical of these hypotheses on the grounds that they ignore a number of institutional changes in the economy over time. To a large extent this is as it should be. After all, we are trying to explain savings characteristics with a minimum of variables. But there are times when underlying changes are of sufficient importance because of a break in their continuity patterns to warrant their explicit consideration. Two particular instances relevant to trends in the United Kingdom may be cited: the high post-war levels of taxation and social security provisions and the recent growth of private pension schemes. To the extent that adjustment problems connected with them were foreseen by individuals they would be allowed for and criticism must be directed against our employment of customary income and savings measures for analysis (*vide* p. 229–30). To the extent that they were unforeseen they can still be fitted into the more flexible Friedman hypothesis at least. Take the case of high estate duties and their discouragement of saving for heirs. Knowledge of these high duties will have induced those concerned to make earlier provision and allocation for meeting this difficulty and to offset taxation, and the main constraints on appropriate action will be associated with borrowing difficulties and associated uncertainties. A variety of assumptions as to the size of the estate provision can be incorporated into the Modigliani model and in this sense it is, as Sargan suggests, better adapted to the explanation of behaviour in older age groups.

The Influence of Assets

Further thought upon the method of inserting assets into the Friedman hypothesis (*vide* p. 241 ff.) after digesting the contributions of Friedman and Tobin suggests the need for qualification of some previous comments. The influence of income variability (P_Y) *via* the associated change in the desired asset/income ratio affects the average propensity to save only insofar as actual asset/income ratios deviate from those desired. To explain the high average propensity to save of self-employed on these grounds requires the additional assumption that any deficiency in the actual asset/income ratio relative to that desired is more marked for self-employed than for others. The speed of adjustment coefficient γ, may of course also vary across groups. This seems reasonable but it obviously cannot be made to bear all the weight imposed on it on p. 233. Some of the explanation must be sought elsewhere, principally, as a number of writers suggest, in the higher expected return anticipated on funds invested by self-employed in their own concerns. This explanation is advanced on p. 238 to explain the magnitude of the marginal propensity to save, but it is of course equally applicable to the average propensity.

By imposing more rigorous assumptions Friedman usefully extends the analysis of the impact of assets on the $\frac{C_P}{Y_P}$ ratio that I derived on p. 242 *et seq.* This indicates the possibility of the estimation of P_Y and P_L which had been a major objective of my original exercise.[1]

Before pursuing this track further we may use Friedman's version of my estimates of b and c (equations 2.40 and 2.41) to comment upon a remark of Tobin's. The latter claims that the negative effects of liquid assets upon savings should, on my arguments, be more pronounced for variable than for stable income occupations. The meaning of this is not clear. It may be the case, as indicated earlier, that γ in equation (2.35) may be greater for groups with higher relative dispersion of transitory incomes. But Tobin refers to the regression equations themselves and here we have to cope with the problem of bias. Using Friedman's version of equation (2.41) we find that c will be equal to γ when $P_Y = 0$ or 1. But when $\beta - \gamma\lambda$ is positive $\left(\text{where } \lambda = \frac{L_P}{Y_P}\right)$ and P_Y lies between 0 and 1, other things equal, c will be larger than γ and greater, the lower P_Y. We shall run the risk of Friedman's wrath by assuming P_Y a sufficient index of the relative dispersion of transitory components. c would be smaller than γ and smaller, the lower P_Y if $\beta - \gamma\lambda$ is negative, but we will ignore this case. However, as we move from stable to variable income occupations λ and even γ may rise, and if Friedman is correct, P_L fall. But it seems impossible to tell the relative degrees of bias short of quantitative estimation. Thus Tobin's theoretical expectations may be right or they may not. Granting him this point, however, we must not overlook the fact that the rate of return on funds may be more attractive for self-employed and that this may be distorting the estimates. This is not to deny that the upshot of this test may well be as Tobin suggests and that total assets, or alternatively wealth, is the relevant variable not liquid assets. This is, once again, a good illustration of the complications of the analysis.

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[1] As Friedman states, I had earlier wished to make these assumptions but had been dissuaded. It transpires that this was due to a misunderstanding over the definition of L. It is only fair to point out that this earlier attempt suffered from another deficiency which I have since eradicated.

Friedman has made some important simplifications to my analysis of the role of assets in his consumption function. As originally outlined the analysis of the influence of assets on consumption was meant to refer to net worth. It was only the paucity of suitable data that led me to apply it to liquid assets too. However, on what limited evidence we have this does not appear over-dangerous. From the 1953 Survey, Lydall has provided figures which show a fair measure of stability in the relationship between liquid assets and net worth.

TABLE I

Age Group	(*1*) *Mean Liquid Assets*	(*2*) *Mean Net Worth*	$\frac{(1)}{(2)}$	*Mean Net Income*	*Mean Number in Income Unit*
18—24	30	40	.75	241	1.2
25—34	90	320	.28	443	2.8
35—44	140	760	.18	509	3.5
45—54	290	1,160	.25	477	2.7
55—64	310	1,200	.26	335	1.8
65 and over ...	280	1,180	.24	200	1.4

Source: H. F. Lydall: 'The Life Cycle in Income, Saving and Asset Ownership", *Econometrica*, April 1955, pp. 131–151.

These figures do not undermine my view that after Age Group 1 liquid assets may be employed as a (proportionate) proxy for net worth. Modigliani's hypothesis under stationary conditions postulates that the asset/income ratio depends on age and family size and not on (permanent) income. Friedman emphasizes that I assume the permanent liquid asset/permanent income ratio is independent of permanent income. Superficially, this assumption appears reasonable though the greater diversification of assets possible as income increases might suggest that this ratio would fall with a rise in permanent income. Greater doubt about the safety of this assumption is cast by the survey of upper income classes in the United Kingdom in 1956.[1]

Friedman demonstrates that if we assume $\frac{L_P}{Y_P} = \lambda$ where λ is independent of Y_P and fit a least-squares equation to $L = cY + d$ we can obtain alternative estimates of P_Y, P'_Y. The results of this operation are set out in Table II. The degree of agreement, which is not close, is better for Managers than for others. In thirteen cases P'_Y exceeds P_Y; in ten P_Y exceeds P'_Y. The discrepancy does not appear to be more pronounced in particular cells, such as age group 3 when income is near a peak, or age group 5 when assets are near their peak. Moreover in two groups, Managers age group 3, and Self-

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[1] See L. R. Klein, K. H. Straw and P. Vandome: ' Savings and Finances of the Upper Income Classes ' BULLETIN, November 1956.

TABLE II

Age Group		*Manual*	*Clerical and Sales*	*Managers*	*Self-Employed*
1	$P_{Y'}$ ...	5.49	1.13	.69	—
	P_Y ...	.97	1.05	1.08	—
2	$P_{Y'}$ ...	1.30	1.22	.89	—.10
	P_Y ...	1.05	.90	.82	.36
3	$P_{Y'}$ ...	.09	.95	1.26	.66
	P_Y ...	1.00	.89	.82	.98
4	$P_{Y'}$ ...	1.84	.42	.70	—.09
	P_Y ...	1.00	.94	.74	.37
5	$P_{Y'}$ ...	1.43	1.17	1.37	.64
	P_Y ...	1.12	1.00	.90	.57
6	$P_{Y'}$ ...	.42	1.21	.85	.52
	P_Y ...	1.06	1.10	1.28	.71

employed age group 4, we have argued that liquid assets/income ratios appeared to be far from normal. This leaves us with at least one perplexing negative sign in the Self-employed column which may be associated with the desire to plough back in view of expected earnings. This may lend weight to Tobin's point that total net worth is the only relevant variable for Self-employed, though results for later age-groups are not too bad. Manual age group 3 yields a very odd result. Yet it remains true that for Managers, where we would expect diversification of assets to be most pronounced, there is a fair degree of consistency in the results, and in Manual, where the reverse applies, there is not. Other influences may be operating. The problem of the negative marginal propensities to save is not satisfactorily resolved and it may not be legitimate to assume $\rho_{L_T Y_T} = 0$. Consumption and assets are not affected in the same way by family size (see p. 226). At very young ages permanent consumption is probably on average higher than mean consumption, and mean permanent liquid assets may also be higher than mean liquid assets. In near-retirement groups, on the other hand, mean permanent[1] consumption is probably higher than mean consumption on average whereas mean permanent liquid assets may be lower than mean measured liquid assets. This seems to help only in the Managerial bracket for young ages, but works unfavourably in age-group 5. In both cases, of course, mean permanent income needs adjustment as in Friedman's point 1. Once again there does not seem to be much room for rejoicing.

Estimates have also been derived for P_L and these are shown in Table III. Friedman's estimates (${}_FP_L$) on the assumption that $\gamma = 0$ are set out below them. Once again there is negligible agreement for Manual but more consistency for Managers and Self-employed. An attempted explanation

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[1] Through this section ' permanent ' is being interpreted in a lifespan sense which may be much too long a horizon. If, in terms only of expected consumption, mean permanent consumption of the near-retired will be lower than mean measured consumption which helps reconciliation in age-group 5.

TABLE III

Relative Variance of Permanent to Total Liquid Asset Holdings

Age Group		Manual	Clerical and Sales	Managers	Self-Employed
1	P_L ...	.03	.03	.07	—
	$_FP_L$ ...				
2	P_L ...	.02	.08	.04	—.01
	$_FP_L$ ...		.27	.12	.06
3	P_L ...	. 00	.15	.13	.05
	$_FP_L$ ...	5.40	.35	.05	.04
4	P_L ...	.02	.03	.12	—.02
	$_FP_L$ ...	.55	—.20	.03	.10
5	P_L ...	.03	.09	.23	.06
	$_FP_L$ ...		1.40	—.73	.004
6	P_L ...	.02	.10	.56	.11
	$_FP_L$ ...				.10

of the negative signs in the latter has already been provided. The most striking feature of the table is the low values of P_L which, in turn, imply a very large relative dispersion of transitory liquid assets. It would be instructive to see the corresponding estimates for each of these tables using total net worth rather than liquid assets. In view of the extreme sensitivity of the Friedman estimates of P_L the deviations between the estimates may perhaps be described as very moderate. Surprisingly enough, the divergencies are greatest for Manual and Clerical and Sales—precisely the cases where we would anticipate $\gamma \rightarrow 0$.

While these results do not inspire much confidence, the method is of more immediate importance. If we could now obtain independent information on P_L, or its corresponding value for net worth, from wealth distributions we should increase substantially the power of these tests and in the process provide a much closer integration between consumption, income and asset studies which have so often been treated as entirely distinct.

Savings and Age and Family Size

Friedman confirms that his hypothesis does not take account of these variables explicitly in the permanent component relationship but emphasizes that I have failed to allow for their influence upon the average propensity to save. This criticism is accepted. But I am not at all sure that one should not argue in like manner that at low and possibly high ages when family size is less than the expected lifetime average, mean measured consumption will be less than mean permanent consumption and that, *ceteris paribus*, this will imply a relatively higher ratio of measured savings to measured income in the extreme age classes. However, even with this counterweight I should rather expect the net effect to be in the direction Friedman indicates.

This leaves open the impact of age and family size upon the permanent consumption/permanent income ratio itself. In line with my extensions of the Modigliani theory I would suggest that this ratio might be expected to vary directly with the difference between the age-weighted current number in the income unit (equivalent adults) and the mean weighted number expected in the unit over the planning period adopted by the unit in determining its ' permanent ' income. Since the Friedman hypothesis is not founded on lifespan planning, one may also anticipate that the permanent consumption/permanent income ratio will be higher in intermediate ages than in extreme age classes, even when we take cognizance of the fact that young wives often continue in employment and that older children start to earn for themselves while they are still, on our definitions, members of their parents' income unit. There is, however, a further influence mentioned by Friedman. To the extent that additional earners serve to reduce uncertainty for the income unit, saving for this reason will be reduced in the upper age classes.

Such an approach, *via* my extension to Modigliani's theory has already been implicit in my interpretation of Table 2.1 (p. 232) but through failure to take account of the effect of age and family size on the measured propensities I understated the support for this aspect of the hypothesis.

My neglect of the age and sex characteristics of the household serves, I believe, to retard the time at which we might expect the $\frac{S}{\bar{Y}}$ ratio to be at its lowest. Marriage should as a rule carry more weight than the arrival of a child and the first child more than a second, etc. This may serve to explain the relatively low ratios of $\frac{\bar{S}}{\bar{Y}}$ for Manual and Managers in age group 2; Clerical and sales, however, have their lowest ratio in age group 3. Why this should have any connection with occupation grouping is not clear, though we do find a lower mean number in the income unit for Clerical and Sales than for other ' working ' groups. Moreover the mean durable consumption/mean income ratio reaches a maximum for Clerical and Sales one period later than for Manual and Managers.

We now turn to Modigliani's comments. In general, Modigliani seems to accept my additions to his hypothesis. However, he effects an important improvement by the more restrictive assumption that γ' and β' in my equation (2.12) are linearly dependent, $\gamma' = \alpha\beta'$. This provides an important interconnection between family size and age that I wish I had seen earlier. As Modigliani emphasizes, my assumptions (see p. 225–6) leave the model somewhat crude. Nevertheless, they appear sufficient for our immediate purpose. Under Sargan's instruction I am now inclined to believe that Modigliani's assumption that an individual (bachelor) plans to consume the same proportion of his expected total resources each year throughout life is appropriate whatever the shape of the income stream, so long as we assume certainty and neglect institutional difficulties. Modigliani uses this in the varying family size case also though his point (i) in the first paragraph of

Section II suggests that he does not regard this as a necessarily natural tendency. While in principle in agreement with Modigliani in his desire to pool the samples of 1952–4 for the purpose of drawing conclusions as to the magnitudes over time of the *average* propensity to save, I am less happy from a practical viewpoint with his results. Firstly, I believe that the results for Self-employed, while undoubtedly culled from too inadequate a sample in my study, are in fact in line with those that would be obtained from a larger sample. That is to say, there is a persistency of relationships of this type observed in other samples (*vide* Friend's comment, for example). Now the retention of Self-employed would not lead to problems on this account if the proportion of effective observations of the various occupation and age groups were exactly the same in each of the years 1952–4 but as Klein, Hill and others have indicated this is not the case. Thus a pooled analysis of these years may introduce a bias into the estimates. Secondly, a further bias is introduced through the different definitions of Savings and Income employed in these years. This reduces somewhat the relative superiority of Modigliani's pooling procedure. Moreover, as Modigliani emphasizes, he gets into difficulty through inability to separate out retired households. These features, to some degree, weaken the case he makes against the employment of the results from the 1953 sample alone. I do agree, however, that it would be advantageous to pool the samples before analysis of the different occupation groups, provided they are based on comparable definitions. In fact, studies from such pooled samples should be carried out. In my view, the retention of Self-employed with their distinct motivation patterns in the comparisons across age-groups may lead to erroneous conclusions. My own position on the inter-age group comparison for age and family size effects is already set out earlier in this section and would seem to carry over with little but terminological adjustment from the Friedman to the Modigliani model. Moreover, for the Manual and Clerical and Sales groups there would appear to be but minor objections (save for the general problems of definition) to the employment of my data in Table 2.1 as they stand. In any event Modigliani's interpretation in Section II seems to accord sufficiently closely with that advanced, albeit inadequately, in my article and in the first portion of this section to make further discussion unwise until more refined and comprehensive data are available. The distinct problems raised by the Managerial group are referred to in another section.

Modigliani's view that current income is not an explanation of the variations of the mean saving/mean income ratios across age groups seems to me correct, though on the basis of my data there is a tendency for the peak $\frac{\bar{S}}{\bar{Y}}$ values to coincide with peak mean income in the case of Clerical and Sales, Managers and Self-employed. The important point is, however, that this can be explained, by influences other than income, namely stages of the life cycle, and in the case of Self-employed, expected profits and their variability.

The case against an absolute income explanation is made more effectively by a comparison of the variation of $\frac{\bar{S}}{\bar{Y}}$ relative to $\bar{Y}$ across age groups.

The inevitable intervention of a coefficient β' into the Modigliani model extended to allow for family size stymied me. Modigliani's very neat assumption that γ' and β' are proportional enables us to proceed much further. However, if I had used his formulation the section which relates to the explicit allowance for variations in family size (J) would not have appeared. As was pointed out, this was inevitably a trial procedure when J enters a regression equation in a complicated manner. But it did in that formulation also appear linearly in its own right (using for example equation A.2 in Modigliani's original article).[1] Now even this seems to be taken from us.[2] The more general approach I adopted, while much less satisfactory than that using Modigliani's simplification, did serve to drive home one or two crucial points. But Modigliani has now managed to probe further and his analysis summarized in his Table 3.1 provides an encouraging degree of support for his thesis. Comment on his analysis in Section III, with which I am in general sympathy, is limited to two points. If the analysis of the Self-employed group from such a small sample were to carry over to larger samples short run income variability might be observed to decline in the intermediate age groups and modify his assumption that v_1 does not change appreciably with age. Secondly, while liquid assets are doubtless a variable proportion of net worth Lydall's data suggest that mean liquid assets are proportionately smallest in age group 3 and in age groups 2, 4, 5, 6 a fairly constant proportion of mean net worth. Modigliani's analysis serves to emphasize just how complex the testing of the ' family ' saving model is. Incidentally his analysis in Section III would seem partially to answer a criticism advanced by Hill (pp. 141-143). Problems such as those raised by Hill are likely to be serious when we cannot adequately control other variables such as age and family size and thus bring into explicit account life-cycle variations.

Friedman's point 7 is too intricate to pursue further at this stage though I feel that the suggestions made at the beginning of this section are particularly relevant to such an enquiry.

Savings and Income

Most of the commentators express dissatisfaction with the adequacy of the tests in this critical section. As mentioned earlier, there are limits to what can be done with a one-period survey alone. No one offers anything very specific as to the further testing that could be undertaken within these limits and instead they produce evidence from other and more varied sources to substantiate their arguments. The two exceptions are a test proposed by Friedman, which Tobin cites, and a test with windfalls given by Klein. The former test is as follows. If in a comprehensive sample successive subclassifications are made by community, occupation, education, family size, age, etc., on Friedman's hypothesis an appropriately weighted average of the income elasticity of consumption (P_Y) from the groups will be smaller than that for the sample as a whole. In fact, as the subdivision proceeds further and further the weighted average should approach zero. It is difficult to execute this test in the present study since the argument implies $0 \leqslant P_Y \leqslant 1$ and in many instances this requirement is not adequately met with our present data (see below). We may, however, test with S' as dependent variable. This would not seem to be so inappropriate for the first age group at least (p. 236). We find that this test is met for the sub-groupings by age and occupation though the fall in the average (weighted as in the sample groups) income elasticity of consumption is quite small. Insofar as even after our subclassifications P_Y remains appreciable, the savings-income regression coefficient will not approach unity. Tobin would appear to expect altogether too much from our subclassification.

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[1] P. 432 *op. cit.*

[2] Through the term $\frac{\bar{Y}_t}{P_t}$.

The second test proposed concerns windfalls. This should be carried out at some stage but in terms of our existing tabulations would require a fresh resort to the questionnaire forms. Windfalls were grouped in the questionnaires under lump-sum receipts which comprised life insurance policies, any other insurance policies, pension funds, gifts, football pools and sweepstakes, post-war credits, etc. It is clear that a number of these are not windfalls in the sense relevant to these hypotheses, *e.g.* maturing endowment assurance policies on one's own life, pension fund benefits after retirement. Nor do I recall that sufficient information was forthcoming to make a clear separation of items that were relevant from those that were not. But speaking from memory many of the items listed were of doubtful relevance.

At this point it is convenient to emphasize again a source of bias in our estimates (see p. 230). Windfalls were deducted from savings totals, but since a sizeable portion of these receipts may have been spent on durables[1] they should probably have been deducted from the total of savings and durables goods expenditure. Our procedure imparts a downward bias to estimates based on S. Moreover, insofar as they are merely a transfer of assets between compartments they should not have been deducted from savings at all, if the variables are defined in accordance with the hypotheses.

While a considerable portion of an unexpected windfall is transitory income, some serves to increase permanent income which in turn raises permanent consumption. (Perhaps this could be approximated by dividing the total windfall by some approximate estimate of the time horizon implicit in permanent income.)

In his contribution Klein makes an attempt to assess for the retired and unoccupied the impact of windfalls on what he calls discretionary savings. These, presumably, are equivalent to our S minus contractual savings for life insurance, pensions and mortgage principal repayment. Windfalls are defined as lump-sum receipts and no attempt is made to separate out pension and similar items. Moreover, State pensions and annual sums from annuities, together with retirement pay, are treated as income, not dissavings from accumulated assets. Thus income, Y, is seriously upward biassed, as over 80 per cent of those in this category are in age groups 5 and 6. A sizeable proportion of windfalls (W) merely represents the conversion of accumulated assets into a form in which individuals can directly finance their consumption in retirement years. To a considerable extent, therefore, W, as here defined does not represent transitory income at all. Until suitable adjustments are made it is hard to see what light Klein's equation sheds on the effect of ' true ' windfalls, *per se*. The sign of W is completely in accordance with expectations if W represents a portion of deliberately accumulated assets now available for financing retirement consumption, though L should not then enter in its own right. Moreover, it should be noted that the coefficient of income is also negative.[1] This example only serves to drive home our earlier points concerning the problem of isolating the net effect of windfalls, as such, on savings. Finally, it should be emphasized that discretionary savings have not been redefined gross of windfalls. If our interpretation of much of the content of windfalls as here defined is accepted, it would seem that on the Friedman hypothesis they have already been completely incorporated into permanent income.

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[1] *Cf.* H. F. Lydall, 'National Survey of Personal Incomes and Savings, Part IV', BULLETIN, Oct.-Nov., 1953, p. 375.

Klein's comment about death duties promoting dissaving only implies that individuals may elect to dispose of all their assets, or the bulk of them, on consumption during their remaining lifespan rather than pass them on in the form of bequests. One can agree to some extent with Sargan that the Modigliani hypothesis may be better adapted to this situation than the Friedman hypothesis.

Klein presents two further equations in which windfalls are included in their own right. These relate to working groups, who presumably are interested in the acquisition of durables. Savings, discretionary or otherwise, should now be redefined gross of windfalls and the saving component of durable expenditure should be added in. Whether W, correctly defined, would exert a negative or a positive influence on savings including the saving component of durables is a question on which these equations are not revealing. Superficially, Klein's results for windfalls argue against the Friedman assumption that transitory income and transitory saving are, on average, perfectly correlated, but the use of survey definitions without adjustment biasses the results in this direction. Some indication of this effect can be gauged by adding $\frac{W}{Y}$ to both sides of the equations. Then $\frac{W}{Y}$ has a positive sign though the quantitative effect of this variable now appears much too strong.

Before we turn to the analysis of savings-income relations by occupation groups we present in Table IV estimates for the *square* of the relative dispersion of permanent and transitory income components calculated in accordance with Friedman's Point 4.

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[1] See Modigliani and Brumberg, *op. cit.*, p. 417, fn. 44.

TABLE IV

Square of Coefficients of Variation of Permanent and Transitory Income Components

Manual	(*S*)		(*S′*)		**Managers**	(*S*)		(*S′*)	
Age	*P*	*T*	*P*	*T*	*Age*	*P*	*T*	*P*	*T*
1	.1481	.0046	.1435	.0092	1			.2043	.0154
2			.0757	.0048	2	.1288	.0283		
3	.1026	—	.1005	.0021	3	.1350	.0296	.1037	.0609
4	.0980	—	.0951	.0029	4	.2024	.0711	.2106	.0629
5			.1370	.0072	5	.2001	.0222	.1890	.0333
6					6			.0907	.0286
All	.1309	—	.1270	.0039	All	.1975	.0463	.1976	.0463

Clerical and Sales					**Self-employed**				
1			.1870	.0038	1				
2	.1694	.0188	.1600	.0282	2	.1255	.2230	.1185	.2300
3	.2372	.0293	.2319	.0346	3	.5034	.0103	.4726	.0411
4	.1790	.0114	.1790	.0114	4	.2446	.0731	.2605	.0572
5	.1526		.1480	.0046	5	.1656	.1249	.1482	.1423
6					6	.3702	.1512	.1721	.3493
All	.3006	.0125	.2881	.0250	All	.3310	.1045	.3266	.1089

P = *Permanent* T = *Transitory*

In general, these estimates suggest a greater relative dispersion of permanent components for Self-employed than for Managerial groups and with the possible exception of age groups 3 and 4 of transitory components also. Moreover, they point to a greater relative dispersion of transitory components for Managers than for Clerical and Sales. As we have already emphasized, they can only ' suggest ' but not ' prove ' since support for this view can only come from a separate investigation of relative income distributions and their properties. As Friedman has indicated, in judging the effect of income variability we should be operating with the relative dispersion of transitory components rather than P_Y. Nevertheless, it does not seem to have led us seriously astray on this occasion. The estimates for Self-employed age-group 3 do rather suggest that I have over-emphasized income variability at the expense of saving accumulation to take advantage of favourable internal rates of return. With these qualifications I still feel that my explanation of the Self-employed in age group 4 looks very reasonable. The relatively large dispersion of transitory components of Managers in age group 4 may indicate substantial promotions at this age, but even if this contributed to the high saving/income ratio it does not help us to account for the high saving/income ratios in certain other Managerial age groups relative to those for Manual and Clerical and Sales. Moreover, as Tobin has correctly deduced from my analysis on p. 242, income variability only serves to account for the saving/income ratio insofar as assets are out of line with those desired as permanent.

The relatively high ratios for Managers as compared with other salaried employees may arise because they expect their employment span to be shorter than that of others or, more generally, because they are uncertain as to how long their services in that role will be desired.[1] This, together with a number of other relevant influences, is stressed by Sargan. Modigliani prefers not to draw deductions from this group because of the inadequate sample size. But we have to use what information we have despite its limited comprehensiveness. Moreover, the estimates are very much in line with those quoted by Friend for the U.S.A. in 1950 and this increases our confidence in them. I am still inclined to believe that expense allowances may serve to inflate the survey measured mean saving/income ratios of this group.

I have argued on p. 232 that if education expenses were included in savings the $\frac{\bar{S}}{\bar{Y}}$ ratios for Managers might be expected to rise with age more steeply. It would seem that the more relevant aspect of education provision for our present problem is that the Managerial group—doubtless partly through their social status—prefer privately to publicly financed education for their children. To finance this they very often take out education policies through life insurance or invest medium term. If this is in fact quantitatively important, it may account in some measure for the saving/income ratios in age groups 3 and 4. But even with all these qualifications one cannot help feeling a little uncomfortable about the support the Managerial group provide for the theories under review. It may be, of course, that we should pay more attention to the changing membership of this group over the lifespan. Pending a more thorough examination the correct attitude of mind would seem to be one of healthy scepticism. This holds equally for an explanation in terms of absolute income.

Both Friend and Klein present evidence from the 1950 Wharton Study in the U.S.A. that is relevant to the testing of these hypotheses. *Prima facie*, Friend's explanation of his Table 2 seems very reasonable, but one would certainly like to see the results excluding the Self-employed. It so happens that at least one year, 1949–50, of the three examined, and possibly two, were recession years in the U.S.A. Thus, both in terms of Self-employed profits and fluctuating incomes of workers through unemployment, one would expect the mean transitory incomes of the group as a whole to be negative and *a fortiori* those in the non-constant income category. This would tend to tilt in the dissaving direction the saving/income ratios of the non-constant income group. In this event it is by no means obvious that these data run counter to the permanent income hypothesis in the upper income brackets of his tables. Furthermore, in line with Friend's comment that the average income of the highest bracket in class A is greater than in class B, it cannot be taken as a point in support of the absolute income hypothesis that, at least in terms of percentages, the saving/income ratios appear lower in *A* than in *B*. In other words, the data in Table 2, in this writer's opinion, provide no presumption against the hypotheses under review. I believe the recession in and about 1949 may serve to explain Klein's Table I. Professor Fellner[1] has pointed out that unemployment reached a peak of 7 per cent of the labour force in the first quarter of 1950, and I presume that there was also a good deal of short time. At a time of unemployment and short time it may be hazardous to predict whether the income variability of the Self-employed or of employees would be the more marked. In line with present arguments, it occasions no surprise to find the saving/income ratio (excluding durables) of wage-earners in U.S.A. in 1950 below Clerical and Sales, and that of the Unskilled even negative: (Friend—Table 1).

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[1] I am indebted to Mr. M. J. Farrell for this suggestion.

In Table II Klein presents evidence from the reinterview survey for 1952–3 in the United Kingdom which strongly suggests that income variability is less marked in Managerial groups than in others. There was in fact somewhat higher unemployment (and presumably short-time) in 1952 than in 1953, particularly in the textile trade, though all through this period unemployment did not exceed 1.8 per cent of the effective labour force. The only other obvious explanation of the higher correlation between incomes in successive years of Managers is that surtax was underestimated in 1952, though this might operate in the direction of raising the coefficient rather than lowering it. It may well be the case that we have to bring one aspect of the ' relative income ' hypothesis to our aid in explaining the high saving/income ratio of this group as compared with other employed groups. Social status in Britain is emphasized by one's ability to send one's children to a public school and high saving/income ratios may be a manifestation of this, but it is also possible that the estate motive plays a more predominant role in this group than in others. These two aspects are, of course, closely connected. In any event it is abundantly clear that age and family size must be taken into account in assessing the ratios for the Managerial group over the earning span.

It is relevant to remark that in a separate study the writer has examined the corporate saving/net profits ratios of U.S.A. corporations[2] and finds a good measure of support for the view that these ratios rise as the trend rate of profits rises. If this carries over to the Self-employed, as it presumably should, the rate of return should occupy at least an equal place with income variability in the explanation of the saving/income ratios of this group. This aspect has inadvertently been under-stressed on p. 233–5, though not by Friedman.

Klein pays some attention to the U.S.A.–U.K. comparison, though why on this issue the writer should incur criticism for taking ' a narrow look ' at manifest data is far from obvious. Institutional differences between countries are important, though one would have expected more when working with conventional definitions of savings and incomes than with definitions in line with those now proposed. This criticism has, one suspects, boomerang effects. From his Table III Klein draws the conclusion that the high marginal propensity to save for the Managerial class is due to their superior income status—not to income variability. One may accept the latter without necessarily accepting the former. In fact we know that those in the Managerial group with £700 or more receive on balance much higher incomes than Clerical and Sales in the same open-ended income bracket. Yet their marginal propensity to save is lower. Nor should I wish to explain this by arguing that the relative dispersion of transitory incomes of Clerical and Sales is the higher.

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[1] E. Lundberg (Ed.)—*The Business Cycle in the Post-War World*, p. 22. See also ' Differential Unemployment and Characteristics of the Unemployed in the U.S.A., 1940–1954 ', by P. M. Hansen, in *The Measurement and Behaviour of Unemployment*, N.B.E.R., 1957.

[2] M. R. Fisher; ' Business Savings and Profits in the light of the Permanent Income Hypothesis ', *Economie Appliquée*, 1957.

Klein stresses that lower income groups reveal insignificant negative correlations between savings and income but that the correlation becomes significantly positive once income rises to about the £800 a year level. Such analyses are in my opinion of little use unless account is taken of age, family size and occupation. After all, a large section of the under £800 group will be young earners and old and retired people who are dissaving. If Klein's results were to hold when these other variables were controlled there would be a strong case for revision of opinion.

There is little doubt that the Managerial group results present a problem of interpretation for these newer theories. But our comparisons across occupations are undoubtedly impeded by our inability to use the appropriate definitions. Pension fund contributions by employers, which affect most strongly Managers, should be included in savings and, on the other hand, we need to make a provision for expense allowances. It is now clear that income variability cannot bear as much weight as I assigned it, but further analysis will have to await the formulation of more exact tests with more appropriate data.

Using re-interview material, Klein presents the results of a test that has received Friedman's blessing. He finds, contrary to his own U.S. experience, that two-year correlation between gross income and the ratio of savings to gross income is higher than that for one year, whereas the permanent-income hypothesis would suggest the converse. This makes a point against the Friedman hypothesis, though one should have liked to see the twelve months' correlation for both 1953 and 1954.

Friedman raises some important questions concerning the elements of my Table 2.5. In particular, he emphasizes that negative marginal propensities to save, as we find in Table 2.4, are not only inconsistent with his hypothesis but also with any other. He then conjectures that the negative signs in Table 2.4 may occur because of the failure to separate primary from secondary income units in a situation where the latter are concentrated at the lower end of the income scale and the former at the top, with secondaries having larger savings on average than primary units. On this view a negative marginal propensity to save for a group composed of primaries and secondaries together could be consistent with positive marginal propensities to save for each alone.

TABLE V

Sign of Regression Coefficient b in $S = a + bY$ *by Age, Occupation and Household Status*

Occupation	*Designation*	*Age Group* 1	2	3	4	5	6
(A) Manual	P(1)	$+$ (11)(94)	$-$ (136)(1127)	$+$ (174)(1554)	$-$ (91)(794)	$+$ (52)(527)	$-$ (21)(195)
	P(2)	$-$ (24)(138)	$-^{1BC}$ (24)(138)	$-^{B}$ (36)(288)	$+^{B}$ (95)(820)	$+^{B}$ (54)(450)	$+^{BC}$ (24)(183)
	S	$+$ (114)(840)	$-$ (61)(482)	$+$ (25)(176)	$-$ (19)(110)	$+^{6B}$ (21)(123)	$+^{B5}$ (21)(123)
(B) Clerical and Sales	P(1)	$+^{AC}$ (14)(109)	$+$ (33)(300)	$+$ (33)(219)	$+$ (27)(171)	$-$ (22)(134)	$+^{C}$ (9)(54)
	P(2)	$-^{2AC}$ (24)(138)	$-^{AC}$ (24)(138)	$-^{A}$ (36)(288)	$+^{A}$ (95)(820)	$+^{A}$ (54)(450)	$+^{AC}$ (24)(183)
	S	$-^{C}$ (79)(563)	$+^{C}$ (33)(210)	$-^{C}$ (13)(89)	$+^{C}$ (13)(89)	$+^{6A}$ (21)(123)	$+^{5A}$ (21)(123)
(C) Managers	P(1)	$+^{AB}$ (14)(109)	$-$ (27)(144)	$+$ (57)(249)	$+$ (37)(173)	$-$ (19)(47)	$+^{B}$ (9)(54)
	P(2)	$-^{2AB}$ (24)(138)	$-^{1AB}$ (24)(138)	$+$ (12)(51)	$+$ (20)(76)	$+$ (14)(64)	$+^{AB}$ (24)(183)
	S	$-^{B}$ (79)(563)	$-$ (17)(61)	$-^{B}$ (13)(89)	$+^{B}$ (13)(89)	N.A.	N.A.
(D) Self-Employed	P(1)	N.A.	$+$ (15)(102)	$+$ (42)(147)	$+$ (33)(138)	$+^{6}$ (19)(74)	$+$ (19)(74)
	P(2)	N.A.	N.A.	$+^{4}$ (18)(63)	$+^{5}$ (18)(63)	$+^{6}$ (17)(61)	$+^{5}$ (17)(61)
	S	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.
(E) Retired and unoccupied	P(1)	N.A.	$+$ (10)(65)	$-$ (11)(85)	$-$ (14)(91)	$-$ (35)(232)	$-$ (170)(1330)
	P(2)	N.A.	N.A.	$+^{4}$ (14)(100)	$+^{3}$ (14)(100)	$-$ (26)(187)	$+$ (90)(657)
	S	$-^{2}$ (15)(84)	$-^{1}$ (15)(84)	N.A.	N.A.	$-$ (14)(109)	$-$ (49)(305)

[1] In certain cases, observations from different cells have been pooled. The superscripts attached to signs indicate the cells with which observations have been pooled; numbers refer to age groups pooled, letters to occupation groups pooled.

Numbers in brackets under sign: first bracket = no. of cases in sample; second bracket = total weight.

P(1) Primaries—1 income unit in household. P(2) Primaries—2 or more income units in household. S Secondaries.

A subdivision by type of income unit and a study of the signs of the regression coefficient has kindly been undertaken for me by Dr. L. R. Klein. The results pertain to the complete sample before the removal of 98 extreme observations listed in Appendix B.

In at most three cells does Friedman's suggestion find clear support—Manual age group 5, Clerical and Sales and Managers age group 6. Manual in age group 2 remain unaffected, while Managers in age group 2 actually yield negative marginal propensities in each income unit subdivision. This underlines Sargan's view that the more one penetrates, the more difficult does explanation become. The negative sign for Clerical and Sales secondaries in age group 1 may be explicable in terms of a preponderance of females dissaving preparatory to marriage. This may also account for the composite group of Managers in age-group 1. A possible explanation of what is happening in Manual and Managers age group 2 is the following. At this stage the weighted family size of the income unit is near its maximum, and this will tend to bring about a consumption peak. Using Modigliani's extended version of the family size model as given in his equation (3.7) and assuming stationary income conditions throughout the earning span with expectations always fulfilled, we see that when J^t_τ is greater than $\bar{J}^t$ the marginal propensity to consume *may* exceed unity, at least for values of α near unity. The same may be true of the average propensity. Table 2.1 supports the latter, while our present table of signs of regression coefficients supports the former. If, at this stage, secondaries dissave more on average than others it would still be possible to obtain a positive regression coefficient for the group as a whole, while negative coefficients occur in each of the sub-groups. Presumably on Friedman's arguments k should not exceed unity. But this assumes that income is defined as the amount an income unit can consume while leaving wealth intact. It may be that at this stage units are not so concerned about keeping wealth intact. The same may apply to the elderly. We might, of course, argue that the horizon for the young is much longer. The mean-consumption/mean income ratio for these groups overstates permanent consumption and understates permanent income so that we may derive a closer approximation to the mean permanent ratios. But at least as far as the regression line is concerned (and probably for both), it does seem that we may have to incorporate the family size effect specifically (as in the case of assets), or alternatively further sub-classify the data, if the general run of my argument is correct. Clearly, in future analyses of these savings data, more attention will have to be paid to the results by types of income unit. It is a pity to have to conclude on such a doleful note.

QUESTIONS OF STATISTICAL METHOD

Heteroscedasticity. In estimation by least-squares methods when the disturbance term is known not to have uniform variance, unadjusted linear estimates can be shown to be unbiassed though inefficient (*i.e.* to have non-minimum variance in small samples). Where the form of the disturbance arrays is known in advance corrections may be made for this, either before or after estimation. Klein finds empirical evidence in the fitting of equations to comprehensive data, as in Part I, that the standard deviations of residuals vary directly with income, though there is some uncertainty about this in the lowest income groups (vide p. 213). He suggests that we specify the disturbance term to be of the form uY. Then by the use of $\frac{S}{Y}$ as dependent variable and adjustments to independent variables, 'best' estimates may be derived. But it is important to realize that this procedure has been chosen on pragmatic grounds, and we have already seen cause to query whether the choice of the form of the disturbance is the most appropriate one. In any event, our support for this procedure arose from the use of economically arbitrary functional forms fitted to comprehensive data.

In Part II, by sub-classification and somewhat closer attention to economic criteria for selection of functional forms, we have reverted to estimating procedures unadjusted for the variance of disturbances. To some extent, we believe, this variance spread will have been reduced. Insofar as it remains an important cause of non-minimum variance in the estimators, we believe that corrections should, of course, still be applied before or after estimation. But we do believe that such adjustments should be made as an integral part of the hypothesis itself. Provided estimates remain unbiassed it might be preferable to use unadjusted estimates until our information on this matter is extended, though this rests on the tacit assumption that samples are 'sufficiently' large.

Using the Friedman hypothesis we can penetrate a little further. We assume that $S_P = (1-k+u)Y_P$ where u has mean value zero and is of constant variance. This is in accordance with the hypothesis that all other factors bearing on savings appear in brackets as a multiple of permanent income. u, then, can serve as a portmanteau for unmeasurable influences and also bear to some degree the weight in our empirical analysis of imperfect classification. A further specification of u would be that it is distributed independently of Y_P, Y_P^2, that uY_P is independent of Y_T, and that C_T is independently distributed of Y_P.

We observe at once that $\bar{S}_P = (1-k)\bar{Y}_P$. For a group for whom these characteristics would appear to apply, the estimate β' in a least-squares regression $S = \alpha' + \beta' Y$ remains $1-kP_Y$.

Given the properties of u no bias is introduced into estimates of β' by this particular specification in the permanent component relationship. This does not settle the question of efficiency; a problem that remains, in any event, when both true (permanent) variables are subject to error (transitory influences) in a regression equation. The variance of residuals in the specified equation above is

$$k^2(1-P_Y)^2\Sigma(Y_P-\bar{Y}_P)^2+\Sigma(S_T-\bar{S}_T)^2+$$
$$+\{[\Sigma(S_T-\bar{S}_T)^2]^{\frac{1}{2}}-(1-kP_Y)[\Sigma(Y_T-Y_T)^2]^{\frac{1}{2}}\}^2+\Sigma u^2 Y_P{}^2$$

The last term of this expression would, on this criterion, suggest a more complicated deflator than that provided by current measured income alone. But Klein and Tobin would presumably argue for adjustment for heteroscedasticity now and for a fine analysis across age and occupation cells of regression coefficients. There is much to be said for this. For this purpose, we should favour a disturbance term of the form uY_P and would be prepared to use either survey net worth or liquid asset holdings as a proxy for Y_P.

The Case For and Against Classification

In a field in which even at this juncture we know so little, there seems to be no overwhelming objection to working at two levels at once. This is the procedure of my paper. In my opinion, the advantages of some classification have been shown in the paper—by the separation of the self-employed or by classification by age. This may be only an interim measure in order to secure greater insight into motivation patterns before attempting an explanation of the variation over the sample as a whole, but it is difficult to see how it can be avoided. It is of course a weakness of the analysis that no tests of the significance of differences of coefficients across cells are presented, though it must be recognized that over-reliance on such ' scientific ' tests, in cases where assumptions are inadequately met, often give rise to undesirable complacency. In Part II of the paper we have tended to reverse the usual procedure by putting first emphasis on economic criteria.

Multicollinearity. In my analysis this term is used to imply a high interdependence among certain independent variables within the sample, not to indicate that some or all of the ' true ' values of independent variables form an exact relationship. This latter point must be guarded against in the specification of the model. On p. 217 my reference to the inclusion of A^2 or J^2 in addition to A and/or J was to the impossibility of assessing their net impact upon the dependent variable. The magnitude of the intercorrelation between A and A^2, as compared with the contribution of A^2 to the explanation of S or $\frac{S}{Y}$, shows without doubt that the standard errors of A and A^2 would be greatly in excess of the point estimates. No more was implied by my comment than this.

Specification of a Stochastic Model

While in general sympathy with Haavelmo's attitude and attack on these problems I find myself unable to divorce so completely theoretical specification from empirical examination. For example, it seems extraordinarily difficult to know how to allow for variations in life-cycle patterns at an armchair level without regular recourse to the data. I therefore subscribe to the view that refinements in model specification must proceed *pari passu* with empirical investigation. Haavelmo's approach does indicate clearly the relation between cross-section and time-series estimates that becomes important in a more embracing study, and it is directly relevant to, and probably indispensable to the interpretation of, Modigliani's use of pooled cross-section samples across time. The use of such specifications has in the past often led to the retention of too great generality in models. The most conspicuous advance of these new theories has been their dependence on very restrictive assumptions which are the more capable of contradiction. One might hazard a guess that if this procedure was followed in other fields such as, for example, international trade, our present microscopic quantitative insight, despite the ultra-refinements of theory, might be greatly augmented.

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ERRATA : Bulletin, Vol. 18, No. 3.

Page 232 : Tables 2.1 and 2.2 ; the heading $\bar{J}-J$ should read $J-\bar{J}$.

Page 243 : Equation 2.41 should read $c=\gamma+\beta'$ {etc.}.